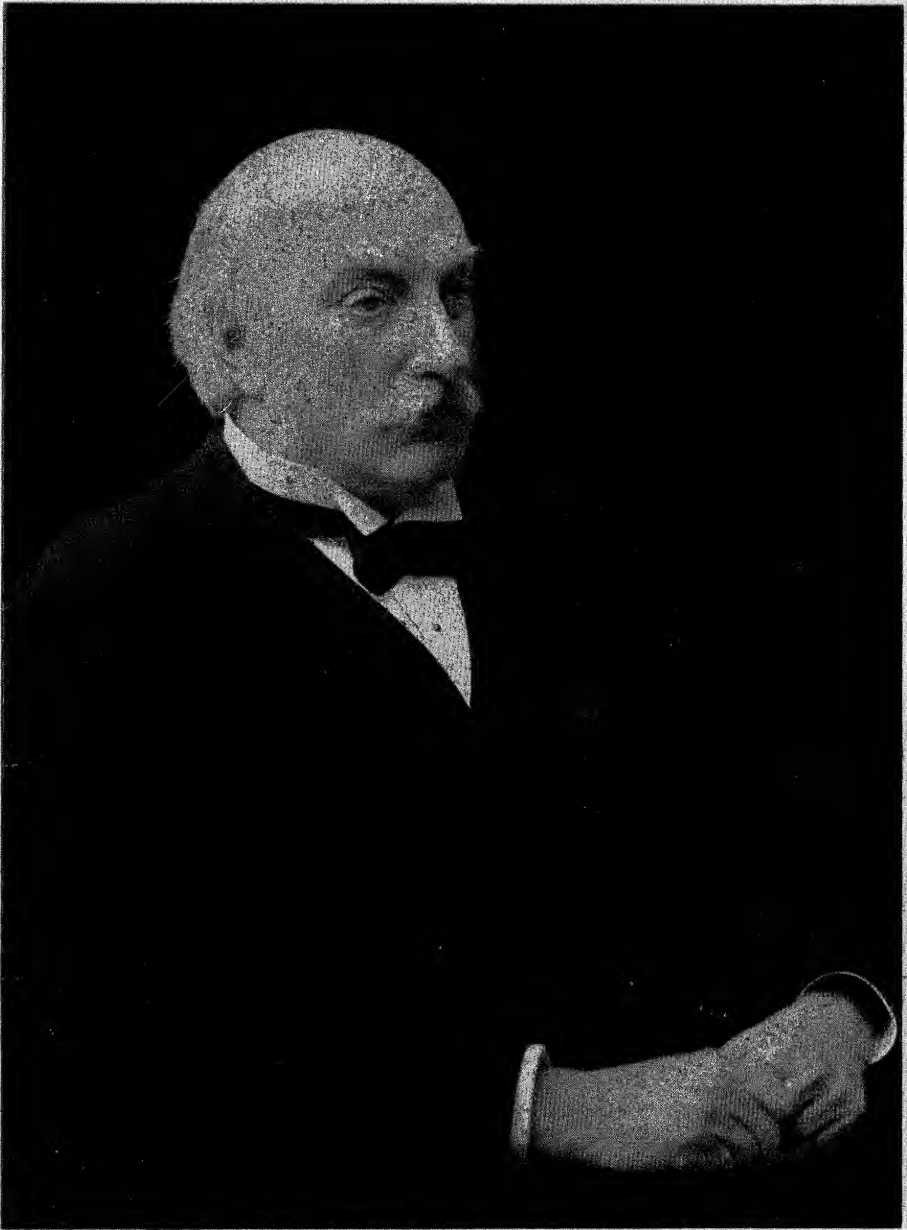


OBITUARY NOTICES
OF
FELLOWS DECEASED.

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Raschke

JOHN WILLIAM STRUTT, BARON RAYLEIGH, 1842—1919.

THE position to be assigned to a man of science in the history of his subject cannot be estimated solely by his published work. The record of that work is permanent; its merit may be judged as well a hundred years hence as it is at present. But there is an invisible element in the fruition of creative power which only the testimony of contemporaries can save from oblivion, because it depends on character as well as on genius and ability. Men have risen to posthumous fame, deserved but dimmed by some shadow of failure because their lives left no impression on the intellectual level of their time. Such men possessed to an exceptional degree that immunity from the influence of current thought which is the essence of originality, but they were the slaves rather than the masters of their own individuality. To receive inspiration as well as to impart it, to stimulate thought at the right moment, to keep in touch with others without losing independence requires human sympathy as well as originality, and it was the combination of the two qualities, each strong in itself and further strengthened by being linked to the other, that has been the outstanding feature of Lord Rayleigh's work. The life of such a man cannot be treated as an independent unit, for it is too intimately connected with the general progress even of those branches of knowledge with which it did not directly concern itself.

It is essential, therefore, to give a brief account of the position of Physics at the time Lord Rayleigh began work. When he took his degree in 1865, the conception of a strictly mechanical construction of the Universe had received strong support through the successes of the undulatory theory of light and the foundation of thermodynamics. Though foreshadowed to some extent in the laws of motion of Galileo and Newton, the principle of the Conservation of Energy had only recently found general acceptance. Men still in the prime of life remembered the days when Joule's experiments were looked upon with distrust, and his views on the nature of heat as heterodox. William Thomson, the future Lord Kelvin, barely forty years old, was in the zenith of his activity, and it was mainly through his efforts that the second law of thermodynamics was formulated while Rayleigh was at school. About the time he entered the University, Maxwell applied the laws of probability to the kinetic theory of gases, and while he was getting ready for his Tripos, the same author's great paper on "A Dynamical Theory of the Electromagnetic Field" was communicated to the Royal Society. The undulatory theory of light had been placed on a sound dynamical basis by Green and Stokes. Though Foucault had disposed of all remaining doubts by showing experimentally that light was propagated through water more slowly than through air, there were some few sceptics still.—Faraday was alive, though deprived of his intellectual powers and nearing his death.

On the purely experimental side, though much valuable work was being done in testing and confirming theories, the discovery of Spectrum Analysis was the only fresh departure in Physics. As such it created as great an interest and was as fruitful in disclosing new facts as the more fundamental discoveries of our own time. The spectra of stars had only just begun to be examined, and the nature of the solar prominences seen during total eclipses was on the point of being revealed. The peculiar luminous appearances observed when electric discharges were made to pass through vacuum tubes had only been incompletely observed, and no attempt had been made to bring them into relationship with other physical phenomena. Photography, as it is now understood, was in its infancy, and many experimental appliances now accepted as almost household utensils were either unknown or of recent origin. The Bunsen burner was only eight years old.

Such was the foundation on which Rayleigh began to build.

The Rayleigh Peerage dates from 1821, when it was bestowed on the wife of Colonel Joseph Holden Strutt as Baroness Rayleigh, in recognition of her husband's eminent services in raising and organising militia regiments. Colonel Strutt sat as M.P. for Maldon from 1790 to 1826, and for Okehampton from 1826 to 1830. It was at his special request that the peerage offered to him was conferred on his wife (a daughter of the Duke of Leinster). When the first Lady Rayleigh died, in 1826, the peerage descended to her son, John James, during the lifetime of his father. He married, in 1842, Clara Elizabeth La Touche, daughter of Captain Vicars, R.E., and on the 12th of November in the same year their first son was born.

The early education of John William Strutt was frequently interrupted by ill health. He entered Eton at the age of ten, but stayed there only for part of a year. Later he went for a short time to Harrow. Ultimately he was sent to Torquay, to be prepared for the University by Mr. Warner, who was the first to recognise his great intellectual powers, and allowed him special privileges in doing his lessons.

The future Lord Rayleigh, having entered Trinity College, Cambridge, as fellow-commoner on Dr. Lightfoot's side, in October, 1861, began his University career apparently without expectations of a high place, but his confidence gradually increased as he proceeded in his studies. His colleagues recognised his ability, and among the pupils of Routh he was looked upon as the most likely to come out as Senior Wrangler. In the annual college examinations he always occupied a high place, though his marks fell considerably short of those who stood at the top of the list. It is to be observed, however, that the examinations included classical and other subjects, and it was understood that in 1864 Strutt was easily first in mathematical papers. Among his recreations were tennis and photography. Though the old wet plate process afforded some opportunities for the practice of manual skill, Rayleigh severely felt the absence of any provision in the University curriculum for experimental work. After taking his degree, he went through a course of chemical analysis under Liveing, but otherwise found no

opportunity to become acquainted with the realities of scientific facts by personal observation.

In October, 1864, Rayleigh obtained the Sheepshanks Astronomical Exhibition, which was open to the competition of all undergraduates. In January, 1865, he took his degree as Senior Wrangler.

The examiners in the Tripos were: William Watson and Michael Marlow Umfreville Wilkinson; the Moderators: Isaac Todhunter and George Richardson. Geometrical Optics took a prominent place in the questions devoted to Mathematical Physics, and there was one question in Physical Optics which deserves mention, because it shows that the possibility of detecting the earth's motion through space by optical means had already been raised, and negatived so far as first order effects are concerned:

"Fresnel supposes that, when bodies move in the medium, the relative velocity of the medium with respect to the body is in the same direction, but in proportion $1:\mu$. Show that the retardation of a plate of glass will be independent of the motion of the earth, if the square of the velocity of the earth to the velocity of light be neglected."

The papers for the Smith's Prize do not reveal any very distinctive features, though more attention is given to contemporary investigations in the questions set by Whewell. One of them deals with the correction of the error of compasses due to the magnetisation of ships, and there is a sting in the tail of another, evidently referring to Lord Kelvin's arguments relating to the history of the earth: "If the heat (*sic*) of the earth's material increases as we descend, what reasoning may be founded upon this as to the history of the earth? How far are these reasonings sound?"

Rayleigh's scientific activity may for convenience be divided into five periods. The division is naturally not a sharp one; work prepared in one of the periods was often published in the next, and, especially during the later years, old problems were again taken up and carried a step forward. The first period extends up to the time when he took up the Cavendish Professorship, and may be taken to include most of the researches contained in the first volume of his 'Scientific Papers.' The second period is dominated by his work on electrical standards. The third period, which bridges over the interval between his departure from Cambridge and the experiments which led to the discovery of Argon, covers to a great extent his tenure of the Secretaryship of the Royal Society. The fourth, or "Argon," period was followed by one of great fertility, but no further distinction can be drawn, and it must be taken to extend to the time of death.

Four years elapsed between the degree examinations and the publication of Rayleigh's first paper. This, and others that followed at short intervals, show an extensive knowledge, not only of the current literature of the problems discussed, but of their historical development. We may already trace in them the chief features of the method of treatment to which he adhered throughout his life. While the subject is looked upon from the most general point of view, the results are illustrated by special applications that

may be, and most frequently are, verified experimentally. The problem is always concisely stated, and the mathematical discussion is reduced to its simplest form by the omission of all that is not important. If we were called upon to define the quality of Rayleigh's work, which forms its characteristic feature and marks his individuality, we should, I think, agree that it lay in his unfailing sense of what is essential in each problem, while he courageously left accessory complications to take care of themselves. Rayleigh himself recognised that his special gifts lay in that direction.

The first paragraph of his paper on "Some Electromagnetic Phenomena considered in connection with the Dynamical Theory" deserves quotation in full, because it is probably the only example in the history of science in which the first few lines, written by a young man for publication, are so typical of the procedure to which he adhered throughout his life :—

"It is now some time since the general equations applicable to the conditions of most electrical problems have been given, and attempts, more or less complete, have been made to establish an analogy between electrical phenomena and those of ordinary mechanics. In particular, Maxwell has given a general dynamical theory of the electromagnetic field, according to which he shows the mutual interdependence of the various branches of the science, and lays down equations sufficient for the theoretical solution of any electrical problem. He has also, in scattered papers, illustrated the solution of special problems by reference to those which correspond with them (at least, in their mathematical conditions) in ordinary mechanics. There can be no doubt, I think, of the value of such illustrations, both as helping the mind to a more vivid conception of what takes place, and to a rough quantitative result, which is often of more value, from a physical point of view, than the most elaborate mathematical analysis. It is because the dynamical theory seems to be far less generally understood than its importance requires, that I have thought that some more examples of electrical problems, with their mechanical analogues, might not be superfluous" ('Scientific Papers,' vol. 1, p. 1).

The paper that follows these introductory lines bears the imprint of the craftsman, marked as clearly as a picture by Perugino carries the signature of the artist in every square inch.

The first illustration of the principle enunciated in the opening paragraphs is derived from an experiment made by De La Rive "in which a battery (such as a single Daniell cell), whose electromotive force is insufficient to decompose water, becomes competent to do so by the intervention of a coil or electromagnet." It is shown that the behaviour of the electrical appliance is closely analogous to Montgolfier's hydraulic ram, and it is pointed out that if the pipe of the ram which provides for the escape of the water were closed, it would be unable to withstand the shock, and, after yielding within the limits of its elasticity, it would burst.

This bursting is compared with the passage of a spark at the place where a circuit carrying an electric current is broken. It will be seen that the explanation of this and other illustrations that are given depends on a true appreciation of electromagnetic momentum, and it must be remembered that at the time it was written there was still a widespread impression that absence of inertia was the characteristic property distinguishing electricity from ordinary matter. The simple manner in which Lord Rayleigh determines the mechanical value of the spark of an interrupted electric current by making use of electromagnetic momentum and energy still deserves to be studied by every student of science.

In a later part of the same paper, attention is drawn to a mechanical theorem of Thomson (Lord Kelvin), which leads directly to the solution of many electrical problems connected with induction. When applied to electrodynamics, the theorem states that whenever a current is suddenly generated in one of the systems, the initial current in all the others is determined by the condition that the energy of the field is a minimum. Interesting illustrations of the applications of this theorem are given.

The latter part of the paper contains the experimental verification of what at first sight looked like a paradox. If a secondary coil be wound round a primary, theory indicated that the initial current in the secondary, induced by the break of the primary current, should be greater the smaller the number of windings in the secondary; the reason being that its self-induction increases as the square of the number of windings, while the mutual induction increases only as the first power. The conclusion cannot be tested by a galvanometer because its indications depend not merely on the measure of the initial current, but also on the rate at which it subsequently falls off. It appeared probable, on the other hand, that the greater initial current might be made apparent through its magnetising effects. After a number of trials, the failure of which was traced to the insufficient suddenness of the break, a condenser was applied at the break like that of an induction coil, and the theoretical result could then be verified.

Most investigators would have been satisfied with this confirmation, but with characteristic caution Rayleigh returned to the subject in a second paper. He looked with suspicion on the introduction of a condenser, because its action was not completely understood. Unknown effects might complicate and vitiate the result. The previous conclusions were therefore confirmed by another experimental arrangement. The concluding paragraphs of the second paper are devoted to a discussion of the action of the condenser in the inductorium, and the opinion is expressed that it is "by no means a complete account of the matter to say that the advantage derived from the use of a condenser depends only on the increased suddenness with which the current is stopped." In a later discussion of the same problem ('*Phil. Mag.*, vol. 2, pp. 280-285 (1901)), which for convenience may be referred to here, a different conclusion is, however, arrived at. Theoretical reasons are given why the condenser can offer no advantages beyond that

of causing a more sudden cessation of the primary current. The subject was verified experimentally, using special devices for interrupting the primary by firing part of it away with pistol or rifle bullets. In one of the experiments described, an Apps coil, with a mercury-oil break and condenser, gave only feeble brush discharges when the spark gap was 60 mm., while without the condenser good sparks were nearly always obtained when the primary was interrupted by a bullet fired from a service rifle.

In 1870 two papers were published in the 'Philosophical Transactions of the Royal Society.' The first is mainly mathematical, the result being applied to the determination of the stationary thermal condition of a sphere exposed to radiation from distant surrounding objects; the second, "On the Theory of Resonance," marks the beginning of that series of memoirs which ultimately was collected and extended in the treatise on "Sound." The importance of that work was at once recognised by no less an authority than Helmholtz, who, in a review of the first volume, wrote:—

"The author will merit in the highest degree the thanks of those who study Physics and Mathematics if he continues the work in the same manner in which he has begun it in the first volume The author has rendered it possible, by the very convenient systematic arrangement of the whole, for the most difficult problems of acoustics to be now studied with far greater ease than hitherto."

It is almost unnecessary to repeat that the feature which renders the two volumes so attractive to the reader, even if he be not specially interested in acoustics, consists in the method which, while illustrating general principles, are at the same time adapted to obtain numerical results in particular cases. This is very noticeable in the paper on Resonance, which forms the first of the series. In conservative vibrating systems of one degree of freedom there is an alternating interchange of potential and kinetic energy. Two constants enter into the mathematical expression of the energies, and once these are determined the natural period of the system is obtained in a simple manner. For air vibrations in cavernous spaces, the dimensions of which are small compared with the wave-length, and which communicate with the external atmosphere by small holes, the problem of determining the period had been solved by Helmholtz, but Rayleigh's method enabled him to find with equal facility expressions for the case where the communication with the external air is no longer by a mere hole, but by a neck of greater or less length. The solution, as has been pointed out, depends on the determination of certain constants. The potential energy is derived from the mechanical value of the compression when the kinetic energy is zero; it depends on the internal volume of the resonator and the compressibility of the gas. The kinetic energy is determined by a method which forms the chief novelty of the paper. The velocity of the air is, of course, greatest near the opening passage, and may be neglected at a short distance from either end of it. The specified condition that the

volume considered is small compared with the wave-length implies that the motion may be considered to be essentially that of an incompressible fluid. Taking these circumstances into account, it can be shown that the constant which is required is identical with what, in the theory of electricity, would be called the electric conductance of the passage supposed to be occupied by uniformly conducting matter. The exact calculation of this conductance presents generally great mathematical difficulties, but by certain assumptions two values, not differing much from each other, may be obtained, one of which gives too high and the other too low an estimate of the pitch.

In the last part of the paper the results obtained are tested by observation. For resonators with one circular aperture, Helmholtz had already obtained an equation agreeing in form with an experimental formula found by Sondhauss, but giving results differing by more than a semitone, or about 7 per cent. This difference would be increased still further if the thickness of the side of the vessel, neglected in the equations, were taken into account. In order to explain the discrepancy, the first question that arose was connected with the method employed to make the resonators speak. Sondhauss blew obliquely across the opening through a piece of pipe with flattened end, but Rayleigh points out several sources of error in this procedure. He was led in consequence to abandon the method of causing the resonator to speak by a stream of air, and to rely on other indications, such as the determination of the note of maximum resonance. This could be done in some cases to a quarter of a semitone by striking a note on the piano and listening to the resonance by means of short indiarubber tubes inserted into the ear and fixed to the resonator. The pitch belonging to small flasks with long necks could be determined by holding them close to the piano, when the resonant note causes a quivering of the body of the flask, easily perceptible by the fingers. With proper precautions in the conduct of the experiments, the theoretical formulæ were always confirmed with considerable accuracy.

After completing the paper, the main results of which have just been described, Rayleigh became acquainted with an experimental investigation of the subject which had been published a few months previously by Dr. Sondhauss in Poggendorff's '*Annalen*.' On the strength of his observations Dr. Sondhauss gave a formula for the pitch of resonators with cylindrical necks. This formula differed in some important respects from that deduced by theory, but in a communication to the '*Philosophical Magazine*,' vol. 40, p. 211 (1870), Rayleigh was able to show that his own rational equations fitted the facts even better than the formula of Dr. Sondhauss.

Several communications to the London Mathematical Society were evidently connected with the preparation of his *Treatise on Sound*. These deal with the vibrations of a gas contained within a rigid spherical envelope, the disturbances produced by a spherical obstacle in waves of sound, and some general theorems relating to vibrations. The last mentioned paper is of

particular interest, as it introduces for the first time the "Dissipative Function" into the general equations of dynamics; but it contains other important results such as "that an increase in the mass of any part of a vibrating system is attended by a prolongation of all the natural periods, or at any rate that no period can be diminished." The principle of reciprocity, particular cases of which had been discussed by previous writers, is considered in its most general aspect.

Rayleigh's attention having been drawn to an echo which returned the sound of a woman's voice with the pitch an octave higher, he was able to give the explanation, founded on his investigations relating to the effect on waves of obstacles small compared to the wave-length. The diverting power of such obstacles was found to vary inversely as the fourth part of the wave-length. The echo referred to was due to a plantation of fir-trees, the trunks and branches of which may be considered as obstacles satisfying the required conditions as regards dimensions. In the reflected or scattered sound, the octave supposed to be contained in the incident wave will be returned with an intensity relatively sixteen times stronger than the fundamental. It may therefore become the dominant note of the echo.

In the year 1877, 'Nature' published a short article by Lord Rayleigh on "Absolute Pitch," which illustrates his singular power of tracing discrepancies to their source. To determine the pitch of a note an arrangement invented by Appunn had come into favour. A series of sixty-five harmonium reeds were tuned so that each when sounding together with its neighbour gave four beats per second. If the pitch of the first be such that the last is exactly the octave of the first, the lowest reed must emit a note of exactly 256 vibrations per second. Proper corrections being made for slight inaccuracies of tuning which reveal themselves by the number of beats between successive reeds being slightly greater or less than four, the arrangement was accepted as irreproachable, but when compared with König's standard a difference of nearly 1 per cent. was revealed. The difference was too large to be accounted for by errors of observation and seemed to indicate some defect in one of the methods employed. The fault lies with Appunn's instrument, but it is by no means obvious. All previous investigators had assumed that the pitch of each reed of the tonometer is the same whether it be sounding together with the reed above or with the reed below. For reasons given in the "Theory of Sound" the assumption is not justified, but there appears to be a mutual influence of the reeds, the sounds repelling each other. This would account for the higher value of Appunn's standard as compared with that of König. The same paper contains the description of a method of comparing directly the frequency of a tuning-fork with that of a pendulum clock. The method gave excellent results and confirmed König's standard.

By another interesting experiment described in the same year, an attempt was made to determine the amplitude of a sound wave in air which lies at the limit of audibility. A whistle was sounded by a constant blast of air so

that the work done per second on the air could be calculated. The necessary data are obtained by measuring the distance between the source of the sound and the point at which it ceases to be audible, and it was thus found that the sound of the whistle could be heard when its amplitude is less than a ten-millionth of a millimetre. On the strength of these experiments Lord Rayleigh expressed the opinion that a sound of the pitch of the whistle (about 2730 vibrations per second) would still be audible when the amplitude is reduced to a hundred-millionth of a millimetre. He confirmed this seventeen years later by an entirely different method, in which tuning-forks were used of frequencies between 256 and 512, the amplitude of the aerial vibration which is just audible being found to be about 5×10^{-9} cms., with a tendency to become less as the pitch rises.

In the five volumes of the collection of 'Scientific Papers,' a number of observations are discussed in nine papers having the title "Acoustical Phenomena." They are chiefly of an experimental character, and I shall have further occasion to refer to some of them, and more especially to those relating to the perception of the direction of sound. At the present stage it is sufficient to draw attention to one important fact brought out in the discussion of the "Æolian Harp" (vol. 2, p. 413).^{*} Previous to the publication of this paper, it had always been assumed that the vibrations of a stretched string sounding under the influence of wind took place in the direction of the wind. Observation, however, showed that this was not correct, the vibrations on the contrary taking place in a plane at right angles to the wind. Rayleigh makes no suggestion with regard to the origin of the forces which set the string in motion, but he found that the wind must be steady in order to maintain the vibration: "It is the irregularity, and not, as has been asserted, the insufficiency of the wind which prevents the satisfactory performance of the harp in the open air." These experiments are insufficiently known, and difficulties have consequently been felt in explaining that the loudness of the sound emitted by telegraph wires under the influence of wind bears no direct relation to the strength of the wind. In the second edition of his 'Treaty on Sound,' vol. 2, § 372, Rayleigh refers to Strouhal's empirical equation connecting the period of vibration with the diameter of the wire and its velocity relative to the air. This equation was based on experiments in which a vertical wire was made to revolve with uniform velocity about a parallel axis. On the assumption that the production of sound depends on the instability of vortex sheets, it was shown that Strouhal's formula may be derived from the theory of dimensions and may be generalized by introducing the coefficient of viscosity. To clear up outstanding questions, Rayleigh had already in 1896 expressed the desirability of extending the observations to liquids. This was actually accomplished by him in 1915, a pendulum free to move in one plane dipping into a vessel of water that could be made to rotate round a vertical axis (vol. 6, p. 300). No motion could be observed when the water flowed in the direction in which

^{*} Unless otherwise stated all references are to the 'Scientific Papers.'

the pendulum was free to oscillate, but it was set into vibrations when the motion of the water was at right angles to that direction. The results obtained were found to be in fair agreement with the theory.

While Rayleigh's work covered almost every branch of Physics, it is generally possible to trace in his consecutive publications the steps by means of which he was led from one problem to another. It would therefore be interesting to be able to fix his starting point. The great attention he paid during the first few years of his scientific activity to the problems of sound may have been due to a predilection for music which frequently accompanies mathematical ability. It probably was so in part; but Rayleigh's general method of attacking problems suggests an alternative explanation at any rate as a contributing influence. There were at the time many important questions in Optics demanding closer investigation, and their difficulties were of two kinds. One is common to all problems involving the origin and transmission of vibrations, the other is connected with the unknown constitution of the medium conveying light and the conditions regulating the different rates of transmission through transparent bodies. It was eminently consistent with Rayleigh's usual procedure to separate the difficulties, and to begin by investigating problems involving vibrations in cases where the mechanical conditions were known and definite.

It is therefore probable that from the beginning he intended to direct his attention to Optics, and in this connection it is of some interest to quote from a letter acknowledging the award of the Rumford Medal of the Royal Society in which he writes that his optical work "has been done perhaps more *con amore* than any other."

At the time when the medium conveying light was supposed to possess the properties of an elastic solid, there was a fundamental question in Optics on which two opposite views were held. Is the direction of the vibration in a polarized ray parallel or at right angles to the plane of polarization? Rayleigh's early papers on Optics endeavoured to elucidate this point from several sides. The first of them deals with the polarization and colour of the light from the sky. Tyndall's experiments had shown that the blue light scattered from small particles was polarized in a direction perpendicular to that of the incident beam. As regards colour, Rayleigh showed that its explanation was simple, for it followed from the principle of dimensions alone that, when the particles were small compared with the wave-length of the incident light, the intensity of the scattered beam must vary inversely as the fourth power of the wave-length. Before working out the theory, the actual prismatic composition of the blue of the sky, as compared with sunlight, was examined experimentally, and found to be in good agreement with the theoretical laws.

The more detailed investigation involving the direction of the vibration depends on the assumption that the scattering particles act in virtue of their greater density by loading the æther so as to increase its inertia without altering its resistance to distortion. The result arrived at shows that the

light should be completely polarized at right angles to the incident beam, and that in order to make theory agree with observation the direction of vibration must be at right angles to the plane of polarization. A further paper on the same subject extends the investigation, and considers the case where the scattering particles differ in rigidity as well as in density from the surrounding medium; the conclusion is arrived at that if both properties vary there is no direction in the plane perpendicular to an incident ray in which there is complete polarization. If there be such a direction—and observation shows that there is—the particles must act either entirely by a change of inertia or entirely by a change of rigidity. In the latter case, however, if the incident beam be polarized, the scattered light should not only vanish in two directions perpendicular to the original ray, but also in four other directions in the plane containing the ray and the direction of vibration, and equally inclined to both. No such polarization is observed, and we are therefore forced to conclude that the particles act through an increase of inertia and do not affect torsional rigidity. In a further communication published in 1881, the subject is treated from the point of view of the electromagnetic theory. Here, again, we have two alternatives, according as the particles act in virtue of introducing variations of electrostatic capacity or of magnetic induction. The second case is excluded for the same reasons that preclude the assumption of an alteration of rigidity in the elastic solid theory. The subject is pushed further in an important respect. All previous results depended on the neglect of powers higher than the first in the supposed alteration of the properties of the medium. In order to find the effect of higher powers, the shape of the particles has to be taken into account. The problem is solved for spherical obstacles, and it is found that the second approximation still maintains one direction of complete polarization, but turns it away from the direction in which the light proceeds. Experiments confirmed this in so far as the maximum polarization could be shown to alter in the manner indicated by theory, but complete polarization was not obtained. This failure is accounted for by the irregularity in the size of the particles, the direction of complete polarization depending on the size. It is further pointed out that the mathematical result only applies to spherical obstacles; with elongated bodies it might be different, and this remark is of importance in connection with the recent investigations of the present Lord Rayleigh.

In a later paper published in 1899 further results of the greatest importance are obtained. The total light scattered by a number of small particles is easily calculated, and the scattering in the direction of the original wave-front is shown to lead to a retardation of phase and therefore determines the refractive index of the medium. Hence a relation may be found between the gradual diminution of intensity, the number of effective particles, and the refractive index.

In these investigations the particles have been supposed to act only in virtue of their increased density or inductive capacity. Modern theories assign the cause of the retardation of phase to the forced vibrations of the

molecule. The effect then depends on the free periods, and, therefore, involves dispersion as well as refraction; but there is no difficulty in showing that the general relation deduced by Rayleigh holds also in this case. The question now obtrudes itself as to whether the scattering particles in the atmosphere are molecules, or whether extraneous matter in the form of dust is the active agent. Using Maxwell's estimate for the number of molecules of air under standard conditions, and Bouguer's value for the absorbing effect of clear air, it was found that about one-third of the total scattering was due to air molecules. A much closer agreement is obtained when more accurate figures are substituted for those available at the time Rayleigh's paper was published. Maxwell under-estimated the number of molecules in a gas, and we now possess accurate measurements for the transparency of air at Washington and Mount Wilson. The average difference between the coefficient of extinction observed on clear days at Mount Wilson and that calculated from the scattering by the molecules of air in different parts of the spectrum is less than 0.5 per cent., and would be practically zero if a small region in the red (probably affected by ordinary absorption) be excluded. It may, indeed, be said that as good a measure of the number of molecules in a gas may be obtained from its refractive index and transparency as by any other method.

Rayleigh's paper of 1881 is the first which is based on Maxwell's theory of light. Up to that date, the formulæ of the elastic solid theory, involving a purely mechanical view of the density and rigidity of the æther, was adopted, and difficulties had to be considered which do not appear in the electromagnetic theory. It may be said in explanation that Maxwell had not specially considered the problem of reflexion. That the surface conditions in the electromagnetic problem lead to expressions which are in better accordance with optical experiments than the elastic solid theory was first pointed out by Helmholtz in 1870, but the brief reference, relegated to a footnote in a long paper, did not attract attention. The mathematical investigations by Glazebrook and Fitzgerald appeared in 1879 and 1880 respectively, while Rayleigh's papers, in which some reference to Maxwell's theory might have been expected, were published in 1871. These papers, written mainly for the purpose of determining the direction of the disturbance in a ray of light, may, as regards their main purpose, perhaps be now considered to have only historical interest; but not entirely so, because the strictly scientific discussion of the propagation of waves in an elastic medium, with well-defined mechanical properties, has applications outside the domain of Optics, and the comparison of the conditions prevailing in an elastic body with that of the imaginary medium conveying electromagnetic disturbances, has still a valuable educational importance. Even if this be not admitted, the historical growth of the present theory of light justifies a brief account of the many contradictions with which the elastic solid theory of light was confronted during the middle of last century. These contradictions were well brought out in Rayleigh's papers, which contain an

eminently clear and fair account of the work of previous investigators. We have already seen that his treatment of the problem of the scattering of light by small particles had led him to the conclusion that the effects of refraction must operate through changes of inertia and not of rigidity. This view had to be tested in other branches of Optics. In double refraction, the same periodic force must suffer a different reaction according to the direction of the force; and it had been assumed by Fresnel and his followers that it is the elasticity that varies. Rayleigh rejects that view on the ground of his previous work and of other fatal objections, which were discussed more fully in a subsequent paper: "We have, then, to consider the question," he asks: "Can double refraction be explained if the statistical properties of the æther are independent of associated matter? Can we suppose that the density within a crystal is a function of the direction of vibration? I answer: Yes. The absurdity is apparent only, and disappears on more attentive examination." He continues his argument by considering—as illustration—an elongated body, such as an ellipsoid, which in the extreme case becomes a circular disc of inconsiderable thickness. If such a disc be suspended in a frictionless medium so that it can be made to perform oscillations in different directions, the medium will have no effect if the displacement be parallel to the plane of the disc. But if it be perpendicular to that plane, the motion of the disc involves a motion of the medium, and the inertia of the moving mass is increased. In a footnote, added in 1899, it is pointed out that Rankine had already in 1851 suggested an inertia different in different directions. Following up this idea, Rayleigh formulated a theory of double refraction, which leads to a wave-surface slightly differing from that of Fresnel. At the time the paper was written, no measurements were available of sufficient accuracy to decide between the two theories; but, shortly after its appearance, Sir George Stokes supplied the deficiency, and gave experimental evidence, which decided the point in favour of Fresnel's wave-surface. This was confirmed in a more complete investigation by Glazebrook.

The reflexion of light from transparent matter was investigated by Fresnel, but it was Green, who, in 1837, first gave a theory based on dynamical principles. A critical discussion of the work of Green, McCullagh, Neumann, and others will be found in Rayleigh's paper published in 1871. When the vibrations are at right angles to the plane of incidence, *i.e.*, parallel to the surface of separation, the mathematical investigation is simple. Assuming the rigidity to be independent of the medium, the amplitude of the reflected wave for unit amplitude of the incident wave was found by Fresnel to be expressed by $\sin(i-r)/\sin(i+r)$, where i denotes the angle of incidence and r that of refraction. If the densities, on the other hand, are supposed to be the same, as in the theories of Neumann and McCullagh, the same amplitude is found to be $\tan(i-r)/\tan(i+r)$. The former expression correctly represents the reflected amplitude for light polarized at right angles to the plane of incidence. Hence, according to Fresnel, the displacements are at right

angles to the plane of polarization, while, according to McCullagh and Neumann, they are parallel to it. But, to complete the theories, it would have to be shown that, for displacements in the plane of incidence, Fresnel's theory would lead to the tangent formula and Neumann's to the sine formula. Their respective authors claimed to have done so, but they obtained their result only by neglecting certain surface conditions which can only be satisfied by introducing surface waves which, though insensible at finite distances, must necessarily affect the amplitude of the reflected wave. Retaining Fresnel's assumption, Green obtained an equation differing from the tangent formula, the discrepancy being small, except near the polarizing angle. Assuming, on the contrary, that the densities are equal, we are led—as Rayleigh shows—to an impossible result, for it would follow that there should be two polarizing angles whenever the difference of refrangibility is small. What interests us here is that he already appreciated in these early papers the close connexion between molecular scattering and the problems of reflexion and refraction, this double polarizing angle having been anticipated by him from his previous investigations, though the mathematical process employed in the treatment of scattering was very different.

It is not necessary to pursue the subject further, because Fresnel's formulæ are easily obtained on the electromagnetic theory, if we treat the media as homogeneous and refraction as being due to differences in inductive capacity. The elliptic polarization accompanying deviations from the tangent formula near the polarizing angle must be due to other causes, among which the oscillatory properties of the molecules, which account for dispersion, demand consideration. There is good evidence that Rayleigh recognised this, for in his treatment of the reflexion and refraction of light by intensely opaque matter, he discusses the effects of anomalous dispersion, and points out that in the increase of refraction on the red side of an absorption band and in its diminution on the violet side, "an analogy may be traced with the repulsion between two periods which frequently occurs in vibrating systems." In a footnote, added in 1899, attention is drawn to an examination question set by Maxwell in the Mathematical Tripos for 1869, showing that he had anticipated Sellmeyer in explaining anomalous dispersion by the reactions of the internal forces of the molecules. As regards the deviations from Fresnel's law, we shall see how at a subsequent stage Rayleigh was led to examine the effects of surface contamination.

The problem of reflexion cannot be completely understood without examining the conditions which are necessary in order that there should be a reflected ray at all. The ordinary theory assumes a definite boundary at which the properties of the medium change abruptly. If the transition is gradual, as in the case of an atmosphere in which the density varies slowly, there is no reflexion. The thinness of the transition layer necessary for the production of reflexion may be expected to have some relation to the wave-length. The problem (vol. 1, p. 460) is treated by Rayleigh in a

simple imaginary case which will assist the student in forming a clear view of the general process.

In the first three years of his scientific activity, the starting point of Rayleigh's investigations was mainly a theoretical problem, and though the clearing up of obscurities or the discussion of difficulties were nearly always supplemented by well-designed experiments, these did not call for any considerable manipulative skill. It is otherwise with his efforts, now to be mentioned, to reproduce optical gratings by photographic methods. That his attention should be directed to this subject at an early stage in his career is of special interest, because this investigation, which at first appeared to have only a practical value, led to a general discussion of the powers of optical appliances, and resulted in the discovery of laws having fundamental importance. Gratings, at the time, were ruled on glass; they were expensive, and differed much in quality. Rayleigh soon gave up the idea which had originally occurred to him of constructing one "on a comparatively large scale and gradually reduce it by the lens and camera to the required fineness." He adopted what he considered the better method by taking a good grating and copying it in the way transparencies for the lantern are printed from negatives. Dry plates, which were then a novelty, were used, and had to be prepared by himself. The reader must be referred to the original papers for a description of the various methods employed, the precautions taken, and the discussion of their relative merits. He will be impressed by the great experimental skill displayed in achieving the success which was soon obtained. The gratings were tested by accurate optical methods, and many of the copies proved to be not inferior in their behaviour to the originals. The investigation led to a closer examination of the theory of gratings. This is simple enough if gratings are treated as made up of a series of openings separated by opaque spaces, but the assumption rarely corresponds to reality, and hence the intensity of light in the spectra of different orders is not, as a rule, what we should expect from the consideration of an equidistant series of openings. Thus it may be found that one of the lateral spectra is stronger than the central image. In most gratings, indeed, what is considered as the opaque part acts merely in retarding the phase of the vibration. "If the grating were composed of equal alternate parts, both alike transparent, but giving a relative retardation of half a wave-length, it is evident that the central image would be entirely extinguished, while the first spectrum would be four times as bright as if the alternate parts were opaque. If it were possible to introduce at every part of the aperture of the grating an arbitrary retardation, *all* the light might be concentrated in any desired spectrum" (vol. 1, p. 215).

The third of four papers on the subject was published in the 'Philosophical Magazine' (1874), and establishes the fundamental theorem for the resolving power of gratings. If a close double line, differing in wave-length by $\delta\lambda$, can just be recognised as such, the resolving power, *i.e.*, the ratio of their

wave-length to their interval as defined by the ratio $\lambda/\delta\lambda$, is shown to be determined by the total number of lines on the ruled surface multiplied by the order of the spectrum. The importance of this theorem lies in the fact that it is not the closeness of the lines but their total number which is effective.

It is convenient to discuss the further theory of gratings in connexion with the general question of the resolving power of optical instruments, which is treated with characteristic thoroughness in a paper, "Investigations in Optics, with Special Reference to the Spectroscope," published in four parts in the 'Philosophical Magazine,' during 1879 and 1880. There is a limit to the resolving power of all optical instruments, owing to the finite length of waves of light. In the case of telescopes, their power of resolving double stars had naturally received the attention of astronomers, and Airy had supplied the mathematical investigation which determines the image of a luminous point formed by a lens. This image is known to consist of a central disc surrounded by rings gradually diminishing in intensity. In 1872, Rayleigh had already pointed out that the diameter of the central image could be reduced, and therefore the resolving power increased, by blocking out the central portion of the lens. Such increase involves, of course, a loss of brilliancy, but this might be advantageous where there is superabundance of light, as in investigations dealing with the sun. The paper was read before the Royal Astronomical Society, Rayleigh's object being to draw the attention of astronomers to a method enabling an observer to protect his eyes against excessive radiation, while at the same time improving the optical efficiency of the telescope. The method is, however, open to the objection that it is not only the eye that ought to be protected but the lens of the telescope, which, when exposed to sunlight, is distorted by unequal expansion due to the absorbed radiation; the present practice favours a partial silvering of the outer surface of the lens for telescopes of very high resolving powers. With smaller powers, when the effect of radiation on the shape of the lens is negligible, Rayleigh's method might still be worth trying.

In the application of the wave theory to the optical powers of prisms and gratings, Rayleigh breaks entirely new ground. If homogeneous light be examined after passage through a prism spectroscope, the width of the image of the slit treated according to the laws of geometrical Optics depends on the orientation of the prism. With equal focal length of collimator and telescope the width of the image is equal to that of the slit only when the prism is in a position of minimum deviation, becoming smaller if the prism be turned in one direction, and larger if turned the opposite way; the dispersion alters at the same time, and hence with wide slits the purity of the spectrum depends on the position of the prism. While attention had been paid to such considerations, it is surprising that no one should have recognised that with narrow slits the laws of geometrical Optics no longer regulate the size of the image, but that—as in the case of telescopes—the purity obtainable is

ultimately limited by the wave-length. Whether viewed through a prism or a grating the width of the central image of a slit illuminated by homogeneous light depends entirely on the difference in the phases of vibration of the two extreme rays. If that difference be equal to a wave-length, the amplitude is zero, and at this point the central image may be said to terminate. It follows in the words of Rayleigh (vol. 1, p. 217), that "if a grating and a prism have the same horizontal aperture and dispersion, they will have equal resolving powers on the spectrum; the greater dispersion is the only cause of the superiority on the diffraction spectra of high order."

In order to express in a mathematical form the limits of resolving power some adequate convention is necessary to define the point at which a double line may be recognised as such. When the principal maximum of the image of one line falls on the first minimum of the other, the combined image has two maxima separated by a minimum, the brightness of which is 0·81 that of the brightness of the maxima. An experienced observer will then have at any rate a strong impression that he is looking at a double line. Taking this as his standard, Rayleigh, as already mentioned, had shown that the resolving power of a grating is equal to the product obtained by multiplying the total number of lines with the order of the spectrum. In the case of prisms the power depends on the dispersive power of the material used and is otherwise proportional to the effective thickness of the material traversed by the extreme ray. "That the resolving power of a prismatic spectroscope of given dispersive material is proportional to the total thickness used, without regard to the number of angles or setting of the prisms, is a most important, perhaps the most important, proposition in connexion with this subject. Hitherto in descriptions of spectroscopes far too much stress has been laid upon the amount of dispersion produced by the prisms; but this element by itself tells nothing of the power of the instrument" (vol. 1, p. 426).

One further point remains to be noted. In order that the full resolving power may be realised, it is necessary that the magnifying power of the optical arrangement should be sufficient to allow the whole of the emergent beam to enter the eye. When the magnifying power is further increased, though there is no loss of resolving power, there is loss of illumination. Nevertheless there is generally an advantage in narrowing the beam until its width is one-third or even a quarter of the diameter of the pupil, on account of the imperfections of the lens of the eye. Rayleigh further discusses the effects of aberrations in optical instruments, the accuracy required in optical surfaces, and concludes his now classical paper by some remarks on the design of spectroscopes. "The general results of the discussion would seem to be in favour of a spectroscope with simple glass prisms of such angle that the reflected light is wholly polarized, the number of prisms being increased up to the point at which mechanical difficulties begin to interfere" (vol. 1, p. 457).

A problem which has important applications in several branches of Physics is solved in a communication to the 'Philosophical Magazine' (1880), under

the title, "On the Resultant of a Large Number of Vibrations of the same Pitch and of Arbitrary Phase." If we consider a space traversed in all directions by radiations of the same period, the question as to the magnitude of the resultant vibration at any one point had been raised by Verdet, who arrived at the conclusion that the resultant of n vibrations of unit amplitude and arbitrary phase approaches the value \sqrt{n} when n is very great. Rayleigh had already shown in 1871 that Verdet's reasoning is incorrect, and in the more complete investigation an expression is obtained for the probability that the magnitude of the resultant lies between any assigned limits. Though the intensity and phase of the resultant is shown to be indefinite, the expectancy of the intensity, *i.e.*, the average intensity in a great number of cases, is found to be equal to the product of the number of combined vibrations and the common intensity.

A question very different at first sight can be shown to be subject to the same mathematical conditions. When a number of detached events, such as earthquake shocks, is treated by the harmonic analysis, the time being the independent variable, the magnitude of the amplitude to be expected for any one period depends on the magnitude of each event exactly in the same manner as in Rayleigh's problem. Another example was supplied by Karl Pearson, who, in a letter to 'Nature' (vol. 72, p. 294), put the following question: "A man starts from a point O and walks l yards in a straight line; he then turns through any angle whatever and walks another l yards in a second straight line. He repeats the process n times. I require the probability that after these n stretches he is at a distance between r and $r+dr$ from his starting point, O." Lord Rayleigh could give the answer at once (vol. 5, p. 256), for it was practically the same problem as that of the composition of n iso-periodic vibrations of unit amplitude and arbitrary phase. Further applications of the fundamental proposition, including its extension to three dimensions were made in papers (vol. 6, p. 565; vol. 6, p. 604) published in the last two years of his life.

The survey of this period of Rayleigh's activity would be incomplete if his important contributions to Hydrodynamics were omitted. The author's index to Lamb's treatise contains sixty references to Rayleigh's papers, and of these more than half were published before 1880. We may note more especially his clear discussion of the "vena contracta" and the "solitary wave." An appreciation of this work dealing with the science of Hydrodynamics from the pen of Prof. Lamb will be quoted further on.

In the account which has been given, and which is by no means exhaustive, of the first ten years of Lord Rayleigh's scientific work, nearly every branch of Physics has been touched upon; yet, as has already been pointed out, the subjects treated are not a mere collection of enquiries which flit haphazard from one subject to another. Occasionally a casual observation or an obscure passage met in the course of reading suggested some point that wanted elucidating, but taken as a whole, the width of range was consequential on the generality of his outlook. In a sense it was more apparent than real.

For to one who primarily considered the fundamental principles on which the science of Physics was then built, and who was constantly on the look-out for applications of these principles, the division of Physics into departments could have but little meaning. There would be no difficulty, were it necessary, to trace the connecting threads which run through the greater part of Rayleigh's theoretical writings and experimental researches. But this might leave a false impression, for no one was more apt than Lord Rayleigh to utilise the opportunities that came to him from the circumstances of his life, or the discharge of his duties, in finding new directions of enquiry. The Cavendish Professorship, the Secretaryship of the Royal Society, the Presidency of the Royal Society all afforded such opportunities, which were used to the best advantage. To appreciate his power of work and devotion to science, it must be remembered that the first period of his career was interrupted by at least one serious illness, and that he had to fit up his own laboratory and carry out single-handed the manual work and often the construction of apparatus required in his experiments.

When Clerk Maxwell died on November 5th, 1879, Lord Rayleigh, by general consent, was invited to succeed him as head of the Cavendish Laboratory, and his acceptance of the post was welcomed by all who were anxious for the future of Physics at Cambridge. He was elected on December 12th, 1879, and entered on his duties at the beginning of the following Lent Term.

The building of the Cavendish Laboratory originated in a report of the Physical Science Syndicate of Cambridge University, which recommended the establishment of a Professor and Demonstrator in Experimental Physics, estimating the cost of a suitable building to be £6,300 inclusive of the apparatus required. The scheme threatened to fail for want of financial support, when the seventh Duke of Devonshire, then Chancellor of the University, offered to provide the necessary funds. After the plans had been drawn out, it was found that the lowest estimate for the building was £8,450, and the Duke generously offered to defray all expenses, including the sum required for equipment. The laboratory, opened in 1874, was thus provided with apparatus necessary for lecture demonstrations and a number of instruments in common use for experimental work. Maxwell thought the Duke "had done enough"* and a year or two later expressed the opinion that the gift should be considered as completed.

The original equipment being inadequate, Lord Rayleigh found it necessary, partly for the purposes of research and partly for the ordinary instruction of laboratory practice, to raise a further sum of money. He contributed £500 himself, and the Duke of Devonshire added another £500 to his previous gift. The total amount subscribed was about £1,500.

Rayleigh gave considerable thought to the organisation of the laboratory as a place of instruction and research, and consulted a few friends who were

* These were the words used in a conversation which remains firmly fixed in my memory.—A. S.

acquainted with similar laboratories in other Universities. But one idea to which he attached importance and which was entirely his own, was to identify the laboratory with some research planned on an extensive scale, so that a common interest might unite a number of men sharing in the work. As a suitable subject he selected the re-determination of electrical standards, and tried to obtain volunteer workers to take part in it. In this he was not altogether successful; partly because the number of sufficiently advanced students was not great, and perhaps also on account of the natural wish of a beginner to try his hand at a problem in which he could show his individual powers. Mr. Horace Darwin, who assisted in the work in its preliminary stages, was prevented from continuing by other occupations. I was then engaged in spectroscopic investigations, but had sufficient faith in the advantage of the general scheme and the importance of the special problem to abandon for a time my own work.

There was a special reason why the determination of the Ohm should be carried out at Cambridge. Clerk Maxwell was closely connected with the experiments on the C.G.S. unit of resistance carried out by a Committee of the British Association at King's College, London, in 1863, and the apparatus based on a method devised by Lord Kelvin was in the custody of the Cavendish Laboratory. Some doubts had been thrown on the accuracy of the experiments. Rowland had found that the British Association unit was 1 per cent. too small, while, according to Kohlrausch, it was 2 per cent. too large. It seemed important, therefore, to repeat the experiment.

The method used consists in observing the deflection of a magnet suspended at the centre of a coil which is made to rotate about a vertical axis. Its accuracy depends on a correct estimate of the self-induction and the dimensions of the coil. Though the construction of a new apparatus of larger size was contemplated from the first it was thought desirable to repeat the experiment with the original coil and to introduce only modifications in the arrangements. Without entering into a discussion of the difficulties that were encountered both on the observational and theoretical side, the stroboscopic method for regulating and determining the speed of rotation of the coil deserves to be noticed. On the axis of the instrument a stout card was mounted divided into concentric circles of black and white teeth of equal width. There were five circles containing 60, 32, 24, 20 and 16 teeth respectively. This disc was observed from a distance through a telescope, and an arrangement for affording an intermittent view with the help of two plates of metal, one of which was fixed and the other attached to an electrically driven tuning fork. The two plates were perforated with slits parallel to the prongs of the fork, so that a view of the revolving disc could only be obtained twice in each complete vibration of the fork. The teeth of the disc appear to be at rest if in the interval between two sights one of the teeth has taken the place of another. By means of the various circles a variety of speeds could thus be brought under observation. The motive power was derived from a water-motor and conveyed to the spinning coil

through a string fastened at a convenient distance below the vibrating fork. The observer by applying a slight friction to the string through the hand could maintain a constant speed, increasing or diminishing the friction according as the teeth of the disc were seen to be displaced in one or the other direction. This method designed by Lord Rayleigh proved very successful and during the whole course of the experiments he himself took charge of that part of it. To get a correct value of the speed it is necessary to know the frequency of the fork. The frequency of the standard was obtained by comparing it directly with a pendulum clock according to a method previously used by Lord Rayleigh and already referred to.

One of the minor troubles that for a time interrupted the smooth progress of the experiment was the behaviour of the suspended magnet, which, when the coil was spinning with open circuit, seemed occasionally jerked suddenly to one side. The investigation of the source of disturbance, which was traced to mechanical tremors, was used by Lord Rayleigh to illustrate an important hydrodynamical effect. It is known that a flat body tends to set itself across the direction of any steady current of the fluid in which it is immersed, and the effect may be expected to show itself also when the current is alternating. If the box containing the magnet and mirror be subject to mechanical vibration, the air in the box will move past the mirror and probably execute several vibrations, the effect of which will be additive. The mirror will, therefore, be subject to a twisting force, tending to set it at right angles to the direction of vibration.

A striking experiment in support of the explanation is described by Lord Rayleigh as follows: "A small disc of paper, about the size of a sixpence, was hung by a fine silk fibre across the mouth of a resonator of pitch 128. When a sound of this pitch is excited in the neighbourhood, there is a powerful rush of air in and out of the resonator, and the disc sets itself promptly across the passage." An instrument capable of measuring the intensity of aerial vibrations, based on this principle, was subsequently designed by Rayleigh (vol. 2, p. 90).

The final result of the investigation on the unit of resistance, conducted with the original apparatus, was that the Ohm, as determined by the British Association Committee, is 0.9893 of a true Ohm, and therefore over 1 per cent too small. The error is in the same direction, but even greater than that found by Rowland.

The research was continued with a new apparatus increased in size in the ratio of 3 : 2, and carefully wound so as to avoid uncertainties in the mean diameter of the winding, which had been troublesome in the previous experiments. The result now gave a value of 0.98651 true Ohms for the B.A. unit, or slightly smaller than the preliminary result.

Mrs. Sidgwick, who from the first assisted in the experiments, took a more responsible share in the later observations, and her name appears as joint author in most of the subsequent work on electrical standards. The time had

arrived when the accurate measurement of resistance, current, and electromotive force became a matter of great industrial importance, which rendered the continuance of the work imperative. A discussion of the relative merits of different methods of determining the Ohm which appeared in the 'Philosophical Magazine' (1882) had led Lord Rayleigh to the conclusion that the one associated with the name of Lorenz, of Copenhagen, was that which promised the best results. With an important improvement, the method was applied and led to a result giving 0.98677 Ohm for the British Association unit, in close agreement with the value obtained with the revolving coil. The general adoption of the Ohm as a unit of resistance was delayed by the favour shown in some quarters to the practical standard of Dr. Werner Siemens. This standard, which could be reproduced without great difficulty when no very high accuracy is required, consists in the resistance of a column of mercury, 1 m. long and 1 sq. mm. in section. This unit was known to be smaller than the Ohm by about 4.5 per cent., and it was important to establish the relationship more accurately. The actual unit in use could be compared with the standard Ohm, but it remained to be shown that it conformed sufficiently with its definition, and for this purpose it was necessary to investigate independently the specific resistance of pure mercury. This was done by Lord Rayleigh and Mrs. Sidgwick.

There remained the important question of establishing a standard method to enable the intensity of a current to be determined in absolute units. The unit current is defined by the magnetic field it establishes, and this field may be measured by its effect either on a suspended magnet or on a suspended coil through which a current passes. For reasons given in the paper, 'Scientific Papers,' vol. 2, p. 278, describing the results, Lord Rayleigh, who was again assisted by Mrs. Sidgwick, chose the second alternative. The ultimate object of the investigation was to enable observers to measure currents in absolute units with appliances that are generally available. Current balances are not suitable for the purpose; even if they can be obtained commercially—like those designed by Kelvin—their indications ultimately depend on the comparison with some standard instrument. It was desirable therefore to connect the results obtained with a standard balance with some laboratory method which is easily applied. Any commercial instrument can then be tested and its errors detected. The electrolytic effect of a current gives us a convenient and effective means of supplying the want. If it be known how much silver is deposited by unit current in a given time, observers may, with apparatus in common use, conduct any experiment in which the intensity of a current is required in absolute measure. The current balance designed by Lord Rayleigh possessed many novel features which all tended to increase the accuracy of the result. The difficulties encountered in pushing the limits of reliability of the silver voltameter to a point comparable with that of the current balance were successfully overcome, and the instructions supplied should enable all competent observers to measure their current to less than one part in a

thousand. In laboratories specially devoted to accurate measurement, a considerably higher accuracy has since been obtained.

The silver voltameter has a great disadvantage due to the time that is necessary to obtain deposits which can be weighed with sufficient accuracy. Currents of the order of one ampère require not less than half an hour to accumulate sufficient silver. It is therefore important to have an alternative method which is more rapid. An electric cell of known electromotive force could supply the deficiency, but when these experiments were conducted, no cell that could be relied upon was known. The most promising one was that constructed by Latimer Clark, depending on the electromotive force between pure mercury and zinc. Before it could be generally accepted as a standard, it was necessary to test its permanence, and the conditions under which cells set up by different persons could be relied upon to give identical results. Lord Rayleigh and Mrs. Sidgwick undertook this laborious work and finally carried it to a successful conclusion. When the highest accuracy is required, the somewhat large temperature coefficient of the electromotive force is a disadvantage, and the Weston cell, in which cadmium is used in the place of zinc, is now more generally adopted.

To complete the investigation of standard measurements of electric currents, there remained still one method which it was desirable to examine. A current passing through a helix establishes a magnetic field in its interior, the strength of which, in terms of the dimensions of the helix, can be expressed in mathematical form with sufficient accuracy. According to Faraday's discovery, this force will turn the plane of polarization of light when passing through refracting media such as glass, or bisulphide carbon. The latter substance may be obtained with sufficient purity, and it seemed desirable, therefore, to determine the angle through which, for unit magnetic force, a given length of the liquid would turn the plane of polarization. The research was carried through with the usual care and attention to relevant detail, but inherent disadvantages prevent the method from entering into competition with others that are more easily applied.

When Lord Rayleigh took over the Cavendish Professorship, the number of students was small, and there was little in the way of systematic instruction in laboratory practice. But the need for such instruction was growing, mainly in consequence of the increasing importance of the practical examination in the Natural Science Tripos. Courses were accordingly organised by the newly-appointed demonstrators, R. T. Glazebrook and W. N. Shaw. A greater number of advanced students also soon entered the laboratory, where they received encouragement and assistance from the Professor. His desire to bring all workers into contact with each other has already been referred to in connexion with the attempt to organise a joint research. The common afternoon tea, introduced by him, was an innovation which may appear to be trivial, but the custom, which affords opportunities for informal discussion of scientific matters and encourages friendly personal intercourse, has been copied in many laboratories, and has

served its object so well that its origin deserves to be placed on record. In connexion with the general policy adopted in the conduct of the laboratory, it should also be recalled that soon after his appointment Lord Rayleigh gave authority to admit women students on the same terms as men.

The room in which the common half-hour was spent in the afternoon contained appliances used by Lord Rayleigh in his many subsidiary investigations. Those who frequented the laboratory at the time will remember a number of roughly constructed but efficient optical devices, and the apparatus for studying the electrical influence on water jets. The investigations for which these appliances were constructed were generally connected with some previous work, and intended to clear up some remaining questions that required illustrating or elucidating. The conditions under which water breaks into drops, more especially, was a favourite study to which Lord Rayleigh frequently returned.

The introductory sentence of the first paper he published on the subject in 1879 (vol. 1, p. 361), expresses the origin of the interest he took in this subject: "Many, it may even be said most, of the still unexplained phenomena of Acoustics are connected with the instability of jets of fluid."

In this paper it is shown that there are two causes which may lead to instability in a cylindrical jet of fluid. One of these is dependent on surface tension, while the other is dynamical. If the fluid as it escapes through a circular orifice be subject to a slight periodic disturbance, such as may be caused by a tremor, the diameter of the jet contracts and expands alternately, so that the outline of the jet takes a wave-like form. The change of surface tension which then comes into play may either tend to increase or to diminish the disturbance. In the former case, the motion becomes unstable, and the jet loses its continuity. Instability will thus set in when—on the average—the disturbance diminishes the surface, for surface tension will then tend to make it smaller still, and therefore increase the effect of the disturbance. The mathematical investigation leads to the conclusion that the jet is unstable when the wave-length of the disturbance (*i.e.*, the period of the tremor multiplied by the velocity of the jet) is greater than the circumference of its cross-section. The wave-length which leads most rapidly to the disintegration of the jet is about 4·5 times as great as its diameter, or about one and a half times the wave-length at which instability first sets in. The subject is pursued in a number of papers, the later ones taking account of viscosity, and from the many instructive experiments arising out of it we may select those referring to the well-known electrical influence on jets as being of special interest. A vertical jet, not subject to any electrical influences, resolves itself into drops which are widely scattered before reaching the summit of their trajectories. When a feebly electrified body is then brought into the neighbourhood of the jet, it undergoes a remarkable transformation, and appears to become coherent. Rayleigh showed that the scattering is due to the rebound of the drops after mutual collisions, which must necessarily occur. In an

electrified field the drops, owing to electrostatic induction, attract each other. In consequence of this attraction, a collision between two drops leads not to a rebound but to a fusion, and hence the scattering is prevented. The exact manner in which fusion takes place has been the subject of some discussion, but adverting to alternative explanations Rayleigh, after some hesitation (vol. 2, p. 117), returns to his original conclusion of the fusion being assisted by the condenser action between the drops (vol. 4, p. 422). The experiment only succeeds when the electric forces are small, because as each drop carries away a charge from the main stream, mutual repulsion will overbalance the attraction if the charges are sufficiently great, and the scattering will consequently increase. When the subject was further examined, it was found that certain substances, such as milk, containing greasy matter, when mixed with water prevent the scattering, the jet reaching the summit in an unbroken stream, and interesting effects were observed when two independent jets were made to collide. The papers dealing with this subject were soon followed by one in which Laplace's theory of Capillarity was improved in an important particular relating to the intense molecular pressure inside solid and liquid bodies.

The second, or dynamical cause, which may produce instability, operates when the jet and surrounding medium have similar properties, so that the effects of surface tension become negligible. Such is the case when a jet of air or some other gas is projected into the atmosphere. The subject was studied by Rayleigh, more especially in connexion with flames sensitive to sound, and his investigation supplied important contributions both on the experimental and theoretical side.

Almost from the beginning of his scientific career, the theory of colour perception attracted Rayleigh's attention. As early as 1871, he communicated to 'Nature' a note on colour vision. It was based on a repetition of Maxwell's experiments, in which colours were combined and matched by using coloured papers attached to and forming sections of a rapidly revolving disc. Rayleigh contributed some pertinent comments on the bearing of the experiments on the theory of trichromatic vision. The great surprise to a novice, who begins to study the superposition of physiological colour sensations (as contrasted with the mixture of paints), is the artificial production of yellow by the combination of red and green. To observe the effect in its full brilliancy, pure spectral colours must be used. When these are not available, Rayleigh recommends an alkaline solution of litmus, which absorbs the yellow and orange, with the addition of chromate of potash, which removes the blue and bluish-green. If the relative proportions of the two substances be properly adjusted, a compound yellow may be obtained. In a further note to 'Nature' (vol. 1, p. 542), an apparatus is described which enables an observer to determine the proportion in which the spectral red and green must be combined to match a pure yellow. A remarkable peculiarity, which affects some persons and appears to be hereditary, was thus discovered. While out of twenty-three male observers,

sixteen obtained results that were consistent with each other, five, of whom three were brothers, required twice as much green to convert red into yellow. The remaining two matched the yellow with less green than the sixteen, who may be supposed to have normal sight. The colour sensation of all women who submitted themselves to the test was normal. It seems highly desirable to secure further statistics on this point. The only attempt made to extend the observations is described by myself in the 'Proceedings of the Royal Society' ('Roy. Soc. Proc.' vol. 43, p. 140 (1890)). Observations on seventy-two persons confirmed Lord Rayleigh's results. There was, however, one woman who had the peculiarity of requiring an exceptional amount of green in matching the yellow. Her husband was normal, but, among three sons, two were affected in the same way as herself.

When Lord Rayleigh resigned the Cavendish Professorship at the end of 1884, the laboratory had established its reputation as a research institution, and he left behind him a number of devoted workers. At the same time, he had gained experience which could not fail to have had a marked influence on his future work. When he came to Cambridge, although he must have been conscious of his great aptitude for designing and carrying out experiments, he began the conduct of delicate and accurate measuring operations with great diffidence. This diffidence, a result of the cautious tendency of his mind, proved to be a valuable factor of safety, preserving him from the danger of over-confidence and keeping him on the constant look-out for unsuspected errors. He never felt satisfied until he had confirmed his results by different methods, and had mastered the subject from all possible points of view. The group of researches, in which he aimed at numerical accuracy, culminated in the discovery of Argon, but ten years intervened between his departure from Cambridge and the ultimate achievement. In the meantime, a number of other investigations demand attention.

Important contributions were made by Rayleigh to certain problems connected with the propagation of waves, more especially in cases where the wave-velocity depends on the period. The most familiar instance is that of deep sea water waves. When a group of such waves advances, we can either fix our attention on a particular wave or on a group as a whole, and we shall find that the group advances with only half the velocity of the wave. The wave passes through the group, and dies out at its front, while fresh waves arise at the tail end. The subject had already been considered by Stokes and Osborne Reynolds, but Lord Rayleigh in his book on Sound, published in 1877, gave a simple formula which allows us to calculate in all cases the group velocity if the relation of the wave-velocities to the wave-length be known. In the particular instance of flexural waves in rods or bars the group moves with twice the velocity of the wave, thus reversing the case of deep water waves. What holds for waves on water or in elastic solids must also hold for light waves, and when we measure the velocity of light by means of the eclipses of Jupiter's satellites, or by Fizeau's toothed wheel, we really determine the group velocity, which, in a dispersive medium, is not identical

with the wave-velocity. It is not quite so obvious that this is equally true when Foucault's method of the revolving mirror is used, but as Willard Gibbs pointed out, the general proposition is maintained also in this case. A more difficult question arises in connection with the aberration of light. In a paper published in 1881, Rayleigh assumed that this depends on the wave-velocity, and therefore provides a practical means of discriminating between wave-velocities and group-velocities in interstellar space, but on reconsideration (vol. 6, p. 41) he acknowledges the justice of Ehrenfest's criticisms, and confirms that the rate of propagation of groups of waves is in this case also the determining factor.

Another application of the theory of wave-propagation was made in 1885. Rayleigh pointed out that waves passing along the surface of an elastic body probably play an important part in earthquakes, inasmuch as, spreading only in two dimensions, their intensity will gain the upper hand at great distances compared with waves spreading through the interior of the earth. This surmise was subsequently fully confirmed, and the Rayleigh waves are now recognised as an important feature in seismograms. Thus in the records of the great Messina earthquake, Prince Galitzin could trace at Petrograd the surface waves which had travelled round in the earth in opposite directions, and from his seismograms deduced their velocity of propagation as being 3.53 kilom. per second. There is a good agreement between this value and that calculated from Rayleigh's formulæ, when plausible assumptions are made with regard to the elastic properties of the materials contained in the upper part of the earth's crust.

Another short paper may here be mentioned, which leads to a result surprising at first sight, but incontrovertible and almost obvious when the conditions of the problem are clearly recognised (vol. 2, p. 417). In what way does the illumination inside a thick cloud or in a fog vary with the distance from the illuminating source or with the direction? The answer is that "at any distance from the source, and whatever the distribution of clouds, there is always in every direction the full radiation due to the temperature of the source, provided only that there be outside a complete shell of cloud sufficiently thick to be impervious."

There is perhaps no better example of the ample harvest so frequently brought to fruition by Rayleigh's generality of vision, concentrating in one focus apparently diverse trains of reasoning, than that furnished in a paper (vol. 3, p. 1), published in 1887. It begins by referring to a previous investigation concerning Melde's experiment in which a string is maintained in transverse vibrations by connecting one of its extremities with the vibrating prong of a tuning-fork. Our mind is then switched off to the Lunar Theory. In Rayleigh's own words: "My attention was recalled to the subject by Mr. Glaisher's address to the Astronomical Society, in which he gives an interesting account of the treatment of a mathematically similar question in the Lunar Theory by Mr. Hill and by Prof. Adams." A few pages are devoted to a differential equation which occurs in the Lunar

Theory, and it is pointed out that a certain constant which, in the astronomical problem, must be real, leads to acoustical applications when it becomes imaginary. The vibrations propagated along a stretched string periodically loaded at equal intervals are discussed, as a simple example illustrating the mathematical results, and the conditions are determined under which the vibrations propagated in one direction ultimately suffer total reflexion. But the acoustical problem is only a step leading to Optics. Periodic structures here are used to explain the brilliant reflected tints such as are seen on superficially decomposed glass. Having started with Acoustics, passed on to the Lunar Theory, and finally to Light, a far-seeing prophecy is relegated to a footnote which must be quoted in full:

“A detailed experimental examination of various cases in which a laminated structure leads to a powerful but highly selected reflexion would be of value. The most frequent examples are met with in the organic world. It has occurred to me that Becquerel’s reproduction of the spectrum in natural colours upon silver plates may perhaps be explicable in this manner. The various parts of the film of subchloride of silver with which the metal is coated may be conceived to be subjected, during exposure, to *stationary* luminous waves of nearly definite wave-length, the effect of which might be to impress upon the substance a periodic structure recurring at intervals equal to *half* the wave-length of the light; just as a sensitive flame exposed to stationary sonorous waves is influenced at the loops but not at the nodes (‘Phil. Mag.’ March, 1879, p. 153; vol. 1, p. 406.) In this way the operation of any kind of light would be to produce just such a modification of the film as would cause it to reflect copiously that particular kind of light. I abstain at present from developing this suggestion, in the hope of soon finding an opportunity of making myself experimentally acquainted with the subject.” To this is added in 1900: “I need hardly remind the reader of the beautiful coloured photographs which M. Lippmann has since obtained by this method.”

Rayleigh accepted the Secretaryship of the Royal Society on the resignation of Sir George Stokes in November, 1885, and held the office during eleven years. Additional duties thus fell upon him, in connexion with which it is appropriate to quote the remarks he made in the Obituary Notice of his predecessor: “And the reader of the Collected Papers [of Stokes] can hardly fail to notice a marked falling-off in the speed of production after this time [the appointment to the Secretaryship]. The reflexion suggests itself that scientific men should be left to scientific work and should not be tempted to assume heavy administrative duties, at any rate until such time as they have delivered their more important messages to the world.” Lord Rayleigh no doubt felt the additional responsibilities of his office, but there was no reduction of output. In his case the number of papers published may serve as a rough measure of scientific production, because he never published without having something substantial to communicate. The following table, in which that number is given for successive

intervals of five years, is therefore of some interest. It will be seen that with the exception of the remarkable period during which he worked at the Cavendish Laboratory, and published at the rate of twelve papers a year, most of them being of considerable importance, his productivity was surprisingly constant:—

	Number of papers published.
Before 1871	5
1871–1875	32
1876–1880	33
1881–1885	60
1886–1890	46
1891–1895	41
1896–1900	45
1901–1905	48
1906–1910	39
1911–1915	52
1916–1919	45

The Senior Secretary of the Royal Society, Sir Michael Foster, no doubt attended to much of the routine work, but it is needless to say that Rayleigh never grudged the time and thought spent on the adjudication of papers presented to the Society for publication.

Perhaps the most important result of his Secretaryship was his rescue of Waterston's important paper that had been relegated to the archives of the Royal Society in 1846. In an introduction to its publication in the 'Philosophical Transactions,' forty-six years after it was read before the Society, Rayleigh explained the circumstances which led him to search for the paper, and expressed the opinion that "the omission to publish it at the time was a misfortune which probably retarded the development of the subject by ten years." A marked service to the history of science was thus rendered by rescuing from oblivion the memory of a somewhat mysterious personality who, after spending a good part of his life as a naval instructor in India, disappeared, one year before Rayleigh took over the Secretaryship, having left his lodgings in Edinburgh for an evening walk, never to be seen again.

It was during the second year of Rayleigh's tenure of office that the 'Transactions' were divided into separate volumes, marked "A" and "B," containing respectively the physical and biological papers. The corresponding change in the 'Proceedings' was made in 1905.

The important experimental investigations on the density of gases which Rayleigh was carrying out during this period might have satisfied anyone less fertile in new ideas or rapid in devising means of submitting them to a crucial test, but many other problems were attacked simultaneously. His researches on capillary forces, both theoretical and experimental,

advanced the subject in many directions, and led to several results of fundamental importance. It has already been described how the study of the instability of jets brought the subject of surface tension to his notice. He continued his investigations in 1882 and 1883, and resumed them again after a further interval of seven years.

The theory of capillary forces indicates that surface tension is a physical constant depending only on the properties of the two bodies in contact. If a plane film of soap solution, such as may easily be produced inside a wire frame, be held horizontally, equilibrium requires, in accordance with the theory, that the tension is the same at every point of the film. Most of us probably have observed without surprise that the film maintains its continuity when placed vertically, though a little reflexion will show that this is irreconcilable with the assumption that, in this case at any rate, tension is independent of the thickness of the film. If the surface forces are the same in all parts of a vertical film, there is nothing to counterbalance gravity and the central portions of the film ought to fall down with the acceleration of a free body. To maintain equilibrium, the tension in the higher parts must be greater than that which is effective in the lower portion. The difficulty here is to bring the facts into harmony with the theory. The simplest explanation, according to Rayleigh, is to adopt a suggestion of Marangoni dating back to 1871, according to which the body of the film is covered by "a coating or pellicle composed of matter whose inherent capillary force is less than that of the mass." It was suggested by Marangoni that the coating was due to contamination from the outside. In a vertical film the pellicle would thicken in the lower portion and would therefore diminish the surface tension to a greater extent than in the upper portions. Rayleigh's first contribution to the subject was suggested by the consideration that if this were true the pellicle would take time to form, and that freshly established liquid surfaces might behave differently. There was already some confirmation of this in Marangoni's observation that within very wide limits the superficial tension of soap solutions, as determined by capillary tubes, is almost independent of strength. The difficulty was to devise some arrangement for measuring the capillary forces in surfaces directly after their formation. This problem was solved by Rayleigh, and the experimental arrangement supplied by his previous researches on liquid jets. When such a jet passes through an elongated aperture, the elongation in the original direction quickly disappears and the cross section becomes circular, to be replaced in its turn by an elongation at right angles, and so alternately backwards and forwards. The jet takes up a chain-like appearance, and the period of the alternate contractions and elongations supplies a means for evaluating the surface tension, which should vary as the inverse square of the wave-length. The experiments completely proved that freshly-formed surfaces of solutions of oleate of soda in water have a considerably higher surface tension than that deduced from the rise in capillary tubes. The latter was about one-third of that observed with water, and remained

constant while the proportion of the oleate was increased from 0.25 to 2.5 per cent. At the same time the surface tension as calculated from the jet increased in the ratio of 5 : 6, being nearly the same in the more diluted solutions as in water. The time required to form the pellicle is of the order of 0.01 seconds.

Once the existence of these pellicles was recognised it became important to examine their constitution and behaviour. The violent movements of bits of camphor scrapings when placed on clean water surfaces are well known. If the surface be greasy the movements are stopped. It was interesting to determine the thickness of a film of oil necessary for the purpose. The quantity of oil was measured by weighing a platinum wire first clean and then charged with a little olive oil. This oil was then spread over a circular surface of water, 33 inches in diameter, and it was found that a thickness of about two-millionths of a millimetre was sufficient to stop completely all movements of the camphor particles. This thickness is equal only to the 300th part of the wave-length of blue light. A further effect of the pellicles is observed in the so-called superficial viscosity of water. This had been investigated by Plateau, who allowed a magnetic needle to swing in contact with water, either when totally immersed or when only its lower surface was in contact with the liquid. In the latter case it came to rest much more quickly. The result of the experiment was that the thickness of the film of oil at which the surface viscosity began to be noticeable is only the sixteenth part of that which is effective in the camphor experiment. A small correction is necessary, because in the observations relating to surface tension special precautions were taken to clean the water surfaces, while tap-water was used in the camphor experiments. The total thickness of oil necessary to stop the movements should, therefore, be raised by the amount which was present before the oil was added, and this Rayleigh estimates to be about 20 per cent. of the thickness of oil that was added.

One of the methods devised by Rayleigh to clean the surface of a liquid depended on the action of an expansible hoop of thin sheet brass. When the hoop is placed in the basin of water so that it includes as little of the water as possible, and then expanded until its shape is circular, the inside surface of the water is freshly formed and should be almost perfectly clean. In a further set of experiments the surface tension of clean water was measured by observing the velocity of propagation of ripples formed by a needle dipping into the water and attached to a tuning-fork. The research presented many difficulties overcome with great ingenuity.

Further papers followed, in which the theory of surface forces is examined critically and historically, and the theory is extended so as to include compressible liquids.

The impossibility of keeping any surface exposed to air free from a film of greasy matter, leads us to expect an appreciable and perhaps important influence in other phenomena in which the surface plays a part. Such is, for instance, the reflexion of light. According to theory, there should be an angle of incidence at which the reflected ray is completely polarized, but

experiment shows that the component which ought to be absent never disappears entirely. An observation was made by Drude in 1889, indicating that the polarization at a freshly split surface of rock salt is very nearly complete, but deteriorates on standing, and this suggested that contamination of the surface was the dominating cause of the discrepancy between theory and experiment. Lord Rayleigh had examined nearly contemporaneously the reflexion from clean liquid surfaces. After a temporary failure, due to a defective optical appliance, Rayleigh returned to the subject in October, 1891, with the result that the polarization at the proper angle of incidence was found to be nearly perfect when the surfaces were thoroughly clean. There is, indeed, in the case of water and some other liquids, a small residual effect in a direction opposed to that observed with surfaces that have been exposed to air.

Returning to the subject in 1908, Rayleigh extended his observations to glass, and again found that, with freshly polished surfaces, the residual polarization in the plane of incidence was reversed. But some unexpected results were obtained with regard to the cause of deterioration. Moisture alone, or even grease, did not appear to have any marked effect, but the glass itself seemed to be attacked by some constituent of the air; the exclusion of carbonic acid retarded the process of deterioration. The paper (vol. 5, p. 489) concludes with the remark: "Surface phenomena generally offer a wide field for investigation, which might lead to results throwing much needed light on the constitution of matter." Experiments with diamond and fused quartz, described in 1912, gave similar results. If it be remembered that the thickness of the film which disturbs the balance upon which the absence of one component at the polarizing angle depends, is a small portion of the wave-length of light, the great delicacy of the optical test becomes apparent.

One further contribution to science, dealing with the increase in the efficiency of a steam engine, which may be expected from superheating or a raising of the boiling-point by chemical means, may be noticed at this stage (vol. 3, p. 538). It suffices to quote the last sentence of the paper in which the subject is discussed: "In conclusion, I will hazard the prediction that, if the heat-engines of the distant future are at all analogous to our present steam-engine, either the water (as the substance first heated) will be replaced by a fluid of less inherent volatility, or else the volatility of the water will be restrained by the addition to it of some body held in solution."

Sensational discoveries are often the result of opportunities well used, but presenting themselves accidentally. Such was the case, for instance, with radioactivity, the first indications of which were due to the accidental passage of a cloud across the sun's disc, which disposed of an erroneous theory, and put Henri Becquerel on the right track. In contrast with such surprises which unexpectedly convert the experimenter into a discoverer by the grace of accident, the new gas "Argon" entered into the family of elements independently of any favourable accident. Its discovery flowed

out of a well-designed series of experiments conducted with a definite purpose, and though the most important of the results arrived at was not anticipated, it came as a well-deserved reward for a strictly scientific procedure which concentrated all possible methods of attack upon one object, perfecting these methods until all discrepancies were cleared up.

It all arose out of the interest, dating back to his Cambridge period, that Rayleigh took in Prout's law, or—as it should be more correctly styled—Prout's hypothesis, according to which all atomic weights are integer numbers. As the exact determination of the relative combining weight of Oxygen and Hydrogen seemed to furnish the most promising test, Rayleigh proposed to determine as accurately as possible the relative densities of the two gases, and to combine the results arrived at with the volumetric analysis of the products of decomposition of water. The latter—as he knew—was being undertaken by Alexander Scott. In a preliminary note published in 1888, he gave, as the most probable result of a set of experiments, 15·884 for the ratio of the densities. A subsequent communication contained the description of experiments in which weighed quantities of hydrogen and oxygen were united by sparking, and the residue measured. This method gave 15·89 for the ratio of atomic weights. After the publication of Prof. Morley's extensive researches on the ratio of volumes, Rayleigh reverted to his original intention of concentrating efforts on the determination of densities, which, though the method was perfected, yielded a number (15·882) only slightly less than that contained in his first communication. Adopting Prof. Morley's results for the volumetric composition of water, the ratio of atomic weights would be 15·880, but, according to later determination by Alexander Scott (quoted by Rayleigh, vol. 4, p. 52), this figure should be reduced to 15·863.

He next turned his attention to nitrogen. We find the first published communication on the subject in a letter to 'Nature,' which appeared in the issue of September 29, 1892. "I am puzzled," he writes, "by some results on the density of nitrogen, and shall be obliged if any of your chemical readers can offer suggestions as to the cause. According to the methods of preparation I obtain two quite distinct values. The relative difference, amounting to about 1/1000 part, is small in itself, but it lies entirely outside the errors of experiment, and can only be attributed to a variation of the character of the gas."

In the first method, the oxygen of atmospheric air was removed in the ordinary way by metallic copper, while in the second method part of the nitrogen was supplied by the decomposition of ammonia. The ammonia made gas was the lighter of the two, and conceivable sources of error through contamination or otherwise failed to explain the discrepancy. The only suggestion in this communication is contained in the query: "Is it possible that the difference is independent of impurity, the nitrogen itself being to some extent in different (dissociated) state?" This letter failed to elicit any valuable suggestion.

If we proceed in chronological order we have next to notice the communication printed in the 'Proceedings' of the Royal Society during 1893, "On the Densities of the Principal Gases." Important improvements were made in the methods used by previous observers. In these the pressure of the gas to be weighed was adjusted by equalising it with that of the outer air, which itself was measured with the help of a barometer. In order to avoid the sources of error that may be introduced by this procedure, Rayleigh placed inside his apparatus a manometer, the principal part of which consisted of a measuring rod carrying steel points at its upper and lower extremities, the pressure being always adjusted so that these steel points were in optical contact with the upper and lower mercury surfaces of the manometer. Throughout the experiments the pressure was therefore rigorously constant. In the reductions, an error that had been overlooked by all previous experimenters was allowed for. When the gas is obtained by comparing the weights of a vessel before and after filling, it never seems to have occurred to these observers that the volume is different in the two cases, owing to the contraction of an evacuated vessel due to outside pressure. The consequent difference in buoyancy may introduce errors amounting to over two parts in 10,000. The figures obtained giving the densities under normal conditions of air for oxygen, nitrogen (including its Argon content), and hydrogen will probably remain for some time to come the most accurate determination of the densities of these gases. The anomaly that occurred with nitrogen prepared from different sources was briefly referred to in these words: "Although the subject is not yet ripe for discussion, I cannot omit to notice here that nitrogen prepared from ammonia, and expected to be pure, turned out to be decidedly lighter than the above. When the oxygen of air is burned by excess of ammonia, the deficiency is about 1/1000 part. When oxygen is substituted for air, so that all (instead of about one-seventh part) of the nitrogen is derived from ammonia, the deficiency of weight may amount to $\frac{1}{2}$ per cent. It seems certain that the abnormal lightness cannot be explained by contamination with hydrogen, or with ammonia, or with water, and everything suggests that the explanation is to be sought in a dissociated state of the nitrogen itself."

In April, 1894, the subject is again referred to in a communication to the Royal Society, in which experiments are described showing that when nitrogen is prepared by passing nitric oxide, nitrous oxide or ammonium nitrate over red-hot iron, the density has the same low value as when it is derived from ammonia. On the other hand, whether the oxygen of the air was removed from the air by hot iron or ferrous hydrate, the higher value held good. It was stated in conclusion that nitrogen prepared from oxygen and ammonia is about $\frac{1}{2}$ per cent. lighter than ordinary nitrogen, and stored in the globe for eight months was found not to have increased in density. This experiment, as Rayleigh subsequently explained in a lecture to the Royal Institution, was intended to test the possibility that the lighter nitrogen included a certain number of dissociated molecules containing only one atom. If this were the

case, it was to be expected that the nitrogen should gradually become denser by recombination.

The question now arose: "What was the evidence that all the so-called nitrogen of the atmosphere was of one quality." On consulting Sir James Dewar, Lord Rayleigh was referred to Henry Cavendish's experiments in which the nitrogen of the air was oxidised by sparking, and after the nitrous oxide had been absorbed by potash, it was found that a residue was left. The conclusion drawn by Cavendish was that if there was more than one inert gas in the atmosphere, the second ingredient could not amount to more than 1/120th part. The only inference that could legitimately be drawn from these experiments was the one adopted by Cavendish, who concluded that "phlogisticated air" (*i.e.*, air deprived of oxygen) was substantially of one kind so far as its power of combining with oxygen was concerned. Whether the small residue left over indicated the presence of another gas was hardly to be settled with the experimental facilities available at the time. But the method had further possibilities under modern conditions. As soon as Lord Rayleigh's attention had been called to these experiments, he set to work and found that, after elimination of the nitrogen, some non-oxidisable gas persisted to show itself in spite of all precautions. The experiments left no doubt that the atmosphere contained a new constituent, though the question as to whether this was a new element or a compound was still open. Excess of cautiousness kept Rayleigh from announcing the discovery, probably on account of the residue not being strictly proportional to the quantity of air used; this was subsequently shown to be due to the absorption of the new gas by water. At this stage Sir William Ramsay joined the research. In his own laboratory he confirmed Lord Rayleigh's work and isolated the new gas, absorbing the nitrogen of the air by means of red-hot magnesium. The discovery was first announced at the British Association meeting at Oxford, and the great paper on "Argon, a new Constituent of the Atmosphere" was communicated to the Royal Society jointly by Lord Rayleigh and Sir William Ramsay on January 31st, 1895. In this paper the various methods of removing the nitrogen from air are examined in detail. It was shown that the new gas, whether prepared by the method of Cavendish, or by magnesium had the same density, within the limits of error; the spectrum of Argon was described, and its solubility in water measured. By a most important series of experiments the ratio of the specific heats was determined, and was found to be 1.66, which proved the gas to be monatomic. It was through this last result that Lord Rayleigh became fully convinced that the new gas was an element. All attempts to discover some chemical reaction of the gas proved abortive: hence the name "Argon" (*Ἀργον*).

At various times Lord Rayleigh took occasion to emphasise with characteristic generosity that the paper in which the discovery of Argon was announced was the joint work of Sir William Ramsay and himself. Without wishing to qualify that declaration, it seems necessary to define clearly the stage at which collaboration began, more especially as some published state-

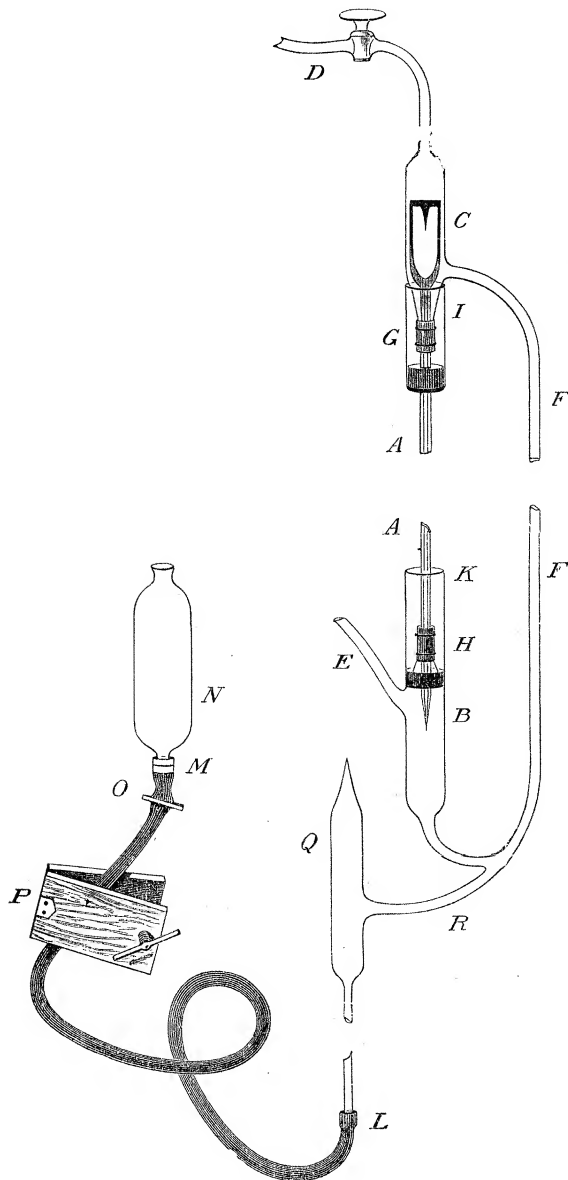
ments accessible to future historians may convey a wrong impression. Lord Rayleigh accepted the assistance of a trained chemist, not because he had come to the end of his own resources, but because the investigation could thus be

carried out more quickly, and in some respects more thoroughly, by including the chemical as well as the physical properties of the gas. Putting aside his own personality, he adopted the course which seemed to him to be most conducive to the progress of science. But the fact remains that he had, without help from others, isolated Argon. He accepted collaboration, but did not ask for it.

In a later communication Lord Rayleigh repeated the density determinations of Argon on a larger scale, and arrived at the figure 19.940 (Oxygen = 16). The refrangibilities of Argon and Helium were determined, as well as their viscosities.

Rayleigh's device of adjusting the pressure of a gas with the aid of a manometric gauge has already been mentioned. It was designed in order to minimise the inaccuracies incumbent on the usual methods of measuring the displacements of a column of

mercury. In these the length to be measured is separated from the scale with which it is to be compared by a thickness of glass which may not be uniform, and hence introduce complications and unavoidable errors. Lord Rayleigh's procedure introduces a novel feature, and marks



what may without exaggeration be called a revolution in the accurate determination of gas pressures. In view of the further extension of the same principle, which greatly increased the range of its possible applications, the gauge in its simplest form is illustrated in the accompanying figure, copied from the first paper on the densities of gases (vol. 4, p. 39). The greater part of the mercury to which the pressure is due is contained in the connecting tube FF. The temperature is taken by a thermometer whose bulb is situated near the middle of FF. B and C are two points which may be said to be the terminals of the steel rod AA. Their distance from each other may be measured accurately before insertion into the apparatus, and they can, by means of a plumb line, be made to lie in the same vertical line. If a vacuum be established in the upper part through D, and, as described in the paper, the points B and C brought simultaneously into optical contact with the two mercury surfaces, the pressure in a vessel connected with E is determined by a column of mercury of known length. No less ingenious than the original device are the modifications which allow the same principle to be applied to changes of volume with changes of pressure. The first of these is described in a paper (vol. 4, p. 511): "On a New Manometer and on the Law of Pressure of Gases between 1.5 and 0.01 millimetres of Mercury." The two mercury surfaces which determine the pressure are now placed side by side in the two branches of a U-tube, instead of vertically above each other. The pointers—made of glass—are adjusted so as to lie in the same horizontal plane. If the upper branches of the U-tube open out to the atmosphere, simultaneous contact shows that the adjustment is correct. Slight deviations are corrected by tilting the apparatus. If one branch of the U is connected to a vacuum and the other to a vessel containing a gas at low pressure, simultaneous contact can again be secured by tilting the apparatus. This was measured optically, and the distance between the points being known to 0.1 of a millimetre, varying pressures up to 1.5 mm. of mercury could be measured to 1/2000 of a millimetre. In two further papers, published in 1902 and 1905 respectively, Lord Rayleigh bridged over the gap between these low pressures, at which Boyle's Law was thus established to a high degree of accuracy and atmospheric pressure. Returning to the original form of the manometric gauge, two of these as nearly similar as possible were used, the similarity, being tested by combining them "in parallel." If the similarity be complete, and they be then used "in series," we are enabled to obtain a pressure exactly double that obtained by employing the gauges separately. The results may be summarised in the following table which gives the value of p_1v_1/p_2v_2 , where $p_2 = 2p_1$, the pressures in millimetres of mercury having values indicated at the heads of the columns. Excess of the figures in the table above unity indicates a higher compressibility than that required to satisfy Boyle's Law :—

Pressures.	380-760.	75-150.
Oxygen	1·00038	1·00024
Hydrogen	0·99974	0·99997
Nitrogen	1·00015	—
Argon	—	1·00021
Air	1·00023	0·99997
Carbonic oxide.....	1·00026	1·00005
Carbonic dioxide.....	1·00279	—
Nitrous oxide	1·00327	1·00066

Lord Rayleigh's earlier work on the theory of optical instruments dealt primarily with telescopes and spectroscopes. In 1896 he extended his investigations to the microscope. The distinctive peculiarity of this instrument consists in the circumstance that the light illuminating the various parts of the object is derived from the same source. Adjacent points of the object cannot therefore be treated as if they were self-luminous, and, if the images of such points partially overlap, there is a phase relation in the overlapping portions which has to be taken into account. Abbé was the first to overcome the difficulty by a novel method of treatment which marked a great advance, and "contributed powerfully to the modern development of the microscope." But the success of this method in bringing out the distinctive properties of the microscope in certain simple cases led to an exaggerated view of its generality. It is, in reality, more limited in its range of application than the older method, which, as Rayleigh showed, is quite capable of dealing with the whole problem. Rayleigh co-ordinated the two methods and discussed their relative merits. The essence of the matter is contained in two significant passages, to be found in the first of the two papers relating to the subject: "In the case of the telescope, we have to do with a linear measure of aperture and an angular limit of resolution, whereas, in the case of the microscope, the limit of resolution is linear, and it is expressed in terms of angular aperture." "It seems fair to conclude that the function of the condenser in microscopic practice is to cause the object to behave, at any rate in some degree, as if it were self-luminous, and thus to obviate the sharply-marked interference bands which arise when permanent and definite phase relations are permitted to exist between the radiations which issue from various points in the object." The second of these quotations is one of the most illuminating remarks that has yet been made on the Optics of the Microscope.

There is no more alluring source of error in physical science than that which springs from an elusive sense of security induced by a mathematical investigation, which is perfectly correct, but rests on certain simplifying assumptions that happen to be unfulfilled in the problem to which it is applied. The great majority of the problems of diffraction which are of interest to the physicist can be explained by elementary reasoning, and he is tempted to apply this same reasoning regardless of the conditions which

limit its application. He is then liable to fall into serious error, from which only a careful study of Lord Rayleigh's investigations can preserve him. Thus in a paper published in 1907 "*On the Dynamical Theory of Gratings*," Rayleigh describes a reflecting grating in which alternate parts are equally wide but so disposed as to form grooves of depth equal to a quarter wave-length. According to the elementary theory, it seems almost obvious that if light fall on such a grating perpendicularly, the central image is obliterated, the whole light being concentrated in the lateral spectra. This is approximately true if the grating interval be wide enough, but it is otherwise when this interval is reduced to less than the wave-length. As Rayleigh remarks: "The conclusion is now entirely wide of the mark. Under the circumstances supposed there are no lateral spectra, and the whole of the incident energy is necessarily thrown into regular reflection, which is accordingly total instead of evanescent. A closer consideration shows that the recesses in this case act as resonators in a manner not covered by Fresnel's investigations, and illustrates the need of a theory more strictly dynamical." The paper supplies that need to a great extent, and incidentally shows how very complicated the complete solution of the problem becomes. The most serious of the complications arise already with a single aperture, and three papers are devoted by Rayleigh to the subject. The first of these deals generally with plane waves of a simple type, which may be waves of condensation and rarefaction, as in sound or electric waves. "Ultimately one or both dimensions of the aperture will be regarded as infinitely small in comparison with the wave-length, and the method of investigation consists in adapting to the present purpose known solutions regarding the flow of incompressible liquids." In two papers dealing more particularly with the light transmitted through narrow slits in infinitely thin perfectly opaque screens (vol. 5, p. 410; vol. 6, p. 161), an experimental result observed by Fizeau is examined and explained. "It appears that if the incident light be unpolarized, vibrations perpendicular to the slit preponderate in the transmitted light when the width of the slit is very small, and the more the smaller this width." With somewhat wider slits, such as scratches upon silvered glass, the opposite polarization prevails. "If the width of the slit were about one-third of the wave-length of yellow-green, there would be distinctly marked opposite polarizations at the ends of the spectrum." But, as the author points out, the conclusion does not hold for ordinary spectroscopic slits, it being one of the conditions of the calculation that the screen containing the aperture should be infinitely thin. In connexion with the same subject we find an interesting Note (vol. 5, p. 405), which supplies the explanation of an observation by Prof. O. W. Wood on a remarkable anomaly in the distribution of brightness in different spectra. At a certain angle of incidence a certain grating was found to show one of the D lines of sodium and not the other. It appears from Rayleigh's discussion of the problem, that the anomaly is connected with the disappearance of a spectrum of higher order. Thus in the spectrum of the first order "an abnormality might be expected at a

particular wave-length, if, in the third order, light of this wave-length were just passing out of the field of view, *i.e.*, were emerging tangentially to the surface."

The science of Optics is rich in the record of observations, which, as already pointed out, fail to be fully explained by the approximate formulæ in common use. We are apt to disregard minor discrepancies, because we feel convinced that more accurate calculations, or a greater perfection of the experimental conditions, would bring the accepted theories into complete accord with the facts. But such partially unexplained phenomena had a peculiar attraction for Rayleigh. Without anticipating any revolutionary new results, he did not feel happy so long as he was without a clear insight into the source of the difficulty, and it often happened that the solution, when once found, had important bearings on related phenomena. As an instance, an observation by Stokes of some peculiar internal coloured reflection in crystals of chlorate of potash may be quoted. It seemed to Rayleigh that a reflexion from a single surface could not account for the bright colours that were observed, but that the structure must be laminated, so as to provide multiple reflexions. This conclusion was not, perhaps, difficult to arrive at, but it led Rayleigh to examine more closely the propagation of light through media possessing a periodic structure. A paper bearing on this question, in which Lippmann's method of colour photography is anticipated, has already been mentioned. Further extensive mathematical investigations of the problem were published by the Royal Society in 1912 and 1917. But all through these investigations Rayleigh was mindful of their bearing on a question in which he was highly interested: the explanation of the vivid colours shown in animal structures such as beetles' wings. In one of the last papers published before his death, he discusses the various theories which have been proposed to account for these colours, maintaining—though with some reserve—his own predilection for the one which depends on interference in the beam reflected from a laminated medium, or one possessing a periodic structure.

While the historical sequence of the problems which attracted Rayleigh's attention has hitherto—so far as possible—been followed, many important investigations were omitted in order to preserve a certain continuity in the subjects that have been discussed. To these we must now return, without pretending to cover the entire ground.

One of the many difficulties of the dynamical theory of gases is the breakdown of the general theorem of equipartition of energy in the case of its distribution among the different periods of radiation emitted by a hot body, and it ultimately required the introduction of an entirely new conception to remove this stumbling block. Without departing from the strictly mechanical ideas which were then universally accepted, Rayleigh examined the nature of the motion which gives rise to white light. The common view of considering such light as being made up of a collection of homogeneous radiations of adjacent periods involves a discontinuity, a continuous spectrum being

regarded as a spectrum of lines which are so close together that our instruments cannot separate them. Though such a view is justifiable in practice, it carries us no further if we wish to obtain an insight into the nature of the molecular movement which originates the continuous spectrum. The alternative is to look on white light—in the manner of Gouy—as being made up of a succession of irregular impulses. In order to account for the distribution of energy between the different frequencies, the impulses cannot be arbitrary: they must possess a finite duration and have a definite structure. An attempt to define that structure is made by Rayleigh (vol. 3, p. 268), with the result that the simplest case which can be treated mathematically leads to a law of radiation which, though it had actually been proposed by H. F. Weber, is not in accordance with observed facts. Eleven years later (1900) Rayleigh showed by simple reasoning that, assuming the law of equipartition to hold, the energy of radiation, within a specified small range of wave-lengths, should be proportional to the absolute temperature, and inversely to the fourth power of the wave-length; and the question is raised whether this may not be the proper form of the law for great wave-lengths. This important suggestion proved to be correct, as was fully established experimentally by Rubens and Kurlbaum. The new idea mentioned above, which accounts for the failure of the law of equipartition in the case of shorter waves, was introduced by Planck, who made the supposition that the energy of radiation is not infinitely divisible, but expressible in terms of a unit or “quantum” which is proportional to the frequency. The unit tending towards zero as the frequency diminishes, the agreement of the intensity distribution for long waves with the theory of equipartition is accounted for.

It is impossible to refer to all the acoustical problems discussed by Rayleigh but some mention should be made of his experiments on the perception of the direction of sound, which were continued at intervals during the whole period of his activity. The problem has a physiological as well as a physical aspect, and Rayleigh pushed its solution to a point at which it must be taken up by the physiologist, or possibly even the experimental psychologist. In 1875 he discovered that an observer could easily recognise whether a sound came from the left or from the right, but with pure notes it seemed impossible to distinguish between front and back. This inability, as was pointed out in a further communication, involves other ambiguities, because if the localisation of sound depends on the ratio of intensities as estimated by the two ears, it follows that for every direction towards the right front there must be a corresponding one towards the right back which is indistinguishable from it. Observations made with the assistance of Sir Francis Galton, who, being deaf in one ear, possessed hardly any power of locating sounds, supported the view that both ears are involved in the process of perception. Two explanations of our sense of sound direction have been offered. The first depends on the sound shadow thrown by the head, which would diminish the intensity of vibration at the ear farthest

from the source. The second explanation assumes some power of distinguishing differences in the phase of the sounds reaching the two ears. The relative merits of the two views were fully discussed by Rayleigh in a paper (vol. 5, p. 347) which formed the subject of the Sidgwick Lecture delivered at Cambridge in 1906. The first explanation, which seems the simplest, satisfactorily accounts for observations with high notes, but when the pitch of the note is such that the frequency is less than 128, the wave-length is several times greater than the diameter of the head, and the difference of intensities at the two ears becomes insensible. For low notes we must therefore fall back on the second explanation, and Rayleigh showed by a series of interesting experiments that a phase difference does indeed induce in our minds a sense of direction, which can be artificially produced if two notes of slightly different pitch are separately led one to each ear. The phase is then alternately ahead on one side and the other, and there is an apparent simultaneous change in the direction from which the sound appears to come. Students of the physiological aspects of acoustics will find much interesting information in Rayleigh's discussion of the manner in which the phase difference may be supposed to act, as also in the two papers in which he describes the results of his experiments on the nature and pitch of sibilants.

In the year 1909 Mr. Asquith, as Prime Minister, appointed an Advisory Committee on Aeronautics to direct the scientific work of a special department to be established at the National Physical Laboratory, and for general advice on the scientific problems arising in connection with the work of the Admiralty and War Office in aerial construction and navigation. Lord Rayleigh was nominated President of the Committee. His interest in the problems of flying dates back to an early period. As far back as 1883 he pointed out in '*Nature*' (vol. 2, p. 194) that a bird cannot, either in still air or in a uniform horizontal wind, maintain its level without working its wings, and that hence the soaring of birds is impossible unless (1) the course is not horizontal, (2) the wind is not horizontal, or (3) the wind is not uniform. "It is probable that the truth is usually represented by (1) or (2), but the question I wish to raise is whether the cause suggested by (3) may not sometimes come into operation." When large birds are observed to maintain their average level in circular sweeps without any motion of their wings, they should rise against the wind and fall with it, if the effective condition of soaring depends on changes of wind velocity with altitude. This was one of the many predictions of Rayleigh that came true, as was shown by an independent observation of Mr. A. C. Baines on the sailing flight of the albatross. A full discussion of the problem will be found in the Wilde Lecture delivered at Manchester in 1900 (vol. 4, p. 462). The reports of the Aeronautical Committee contain several important contributions by Lord Rayleigh. The first of these draws attention to the important conclusions that can be deduced from the principle of "dynamical similarity." By means of it we may interpret the results

obtained with models of dimensions that are manageable in a laboratory, and apply them to cases where the magnitudes of the determining quantities are altered in suitable proportions. The principle of similarity has proved to be of the greatest importance in aerodynamics, and furnishes a scientific method of testing the construction of flying machines.

I am indebted to Prof. Horace Lamb for the following brief appreciation of Rayleigh's work on Hydrodynamics, from which a few passages referring to matters already mentioned have been omitted.

"Rayleigh's work on Sound and the theory of vibrations led by a natural transition to the study of water waves, where the same general principles are involved. His first paper (1876) on the subject deals with the conditions for wave-propagation without change of type, the problem being reduced to one of steady motion. The fundamental results of Airy, Stokes and others are thus obtained by a greatly simplified analysis. The theory of the 'solitary wave,' which had been studied experimentally by Scott Russell, is next investigated, and it is explained in particular why such a wave must necessarily be one of elevation only. In this work he had been anticipated by Boussinesq, who shares the credit of elucidating a matter which had long been a perplexity to mathematicians. The theory of deep-water waves of permanent type, which had been the subject of one of Stokes' classical researches, was also touched upon by Rayleigh in the above paper. It was to have a lasting fascination for him, and he returned to it at intervals almost to the end of his life, continually extending its approximations.

"The cognate theory of the ripples and waves produced when a local disturbance travels over a horizontal sheet of water at a speed exceeding a certain minimum had been outlined by Kelvin in the year 1871. The subject was in turn taken up and completed by Rayleigh, so far as the two-dimensional form of the problem was concerned. He also discussed the form of the ripples produced by a travelling point-disturbance. An analytical artifice which is introduced in this paper, and which subsequent writers have found very useful, may be referred to as characteristic. A mathematical indeterminateness, which presents itself in various problems of steady motion (owing to the implicit inclusion of free waves) when dissipation is neglected, is evaded by the temporary insertion of frictional terms varying as the velocity, whose coefficient is ultimately made to vanish. The indeterminateness is thus avoided, without the necessity of employing the true law of viscosity, with the elaborate analysis which this would involve.

"The theory of discontinuous motions in frictionless liquids had been originated by Helmholtz and Kirchhoff in 1868 and 1869 in two classical papers. The work of the latter suggested to Rayleigh a theory of the resistance encountered by a plane lamina moving through a stream at a constant inclination, and he completed Kirchhoff's solution from this point of view. The results, though not in complete accordance with more recent measurements, were a great improvement on previous explanations, and have stimulated much subsequent investigation. They may still claim, moreover,

to furnish the best *general* representation of the phenomena which we are as yet able to get by *a priori* dynamical reasoning. The friction over the surface of the lamina, and the formation of eddies in the wake, introduce qualifications which are as yet beyond the reach of calculation.

"It had been remarked by Helmholtz, and further insisted upon by Kelvin, that a surface of absolute discontinuity in a frictionless liquid would necessarily be unstable. The question of stability of flow was of continual interest to Rayleigh, originally no doubt from its bearing on various acoustical phenomena, and later for its own sake. His first enquiry was: to what degree is the instability affected if the discontinuity is eased off, as it is in reality by viscosity? He found that the instability remains for disturbances whose wave-length exceeds a certain limit. He further investigated the case of flow between parallel planes, as the two-dimensional analogue of flow in a pipe, having in view Reynolds' experimental demonstration of a critical velocity. The motion proved to be stable provided the graph of the undisturbed velocity, as a function of distance from the medial plane, is free from inflexions. A similar conclusion was reached later for the case of a pipe. It is to be remembered that in these investigations the disturbance of the steady flow is assumed to be infinitesimal. Moreover, although the steady flow may be of the type which could be maintained under the influence of viscosity, the effect of friction on the disturbed motion is in fact neglected. In particular, the condition of no slipping at the walls, which appears to be fundamental, is violated. Calculations of the above type were resumed at frequent intervals, and his more recent papers include a masterly review of the subject, in which viscosity is duly considered. The theoretical problems which were set by Reynolds' experimental work must be regarded as still unresolved, but Rayleigh's papers have done much to clear the ground, and contain besides many points of independent interest."

Rayleigh's remarkable influence on contemporary science is due, in great part, to the example he set by his method of attack and exposition. His mathematical analysis never lost touch with realities, so that the lucid presentation of the nature of the problem, and the simple but precise reasoning along which the argument proceeds, allows even the non-mathematical reader to appreciate the bearing of the results, and the limitations to which the problem is subject. Maxwell once said that everyone is a mathematician, with the difference that some know it and others don't. Rayleigh seemed to have the power of bridging the gulf, and of presenting mathematical arguments in a form that can be appreciated, even if not followed in detail, by the tyro. This uncommon faculty is specially noticeable in his discussion of the law of Partition of Energy (vol. 4, p. 432), one of the most difficult subjects in mathematical physics, yet discussed with such exquisite simplicity and perspicuity that, whether mathematician or experimentalist, we must all feel whenever we return to it, that we have gained some fresh insight into the intricacies of this many-sided problem. Out of numerous similar examples that might be given, it must suffice to refer only

to one of his later papers, in which those interested in Fourier's theorem (vol. 6, p. 227) from the physicist's point of view cannot fail to find much instructive reading.

In his Presidential Address to the British Association, Lord Rayleigh, speaking of future progress in general chemistry, remarked: "And if I might without presumption venture a word of recommendation it would be in favour of a more minute study of the simpler chemical phenomena." It was not only in chemistry that the simple phenomena attracted him. Thus he examined the process of polishing and grinding glass, showing that the latter process acts by breaking out small fragments (pitting) while, as regards polishing, he concludes that (vol. 4, p. 546): "in all probability the process is a molecular one, and that no coherent fragments containing a large number of molecules are broken out. If this be so, there would be much less difference than Herschel thought between the surfaces of a polished solid and of a liquid." Simplicity was his great aim, not only in choosing the problem, but also in the procedure adopted to solve it. What could be more simple or more effective than the method designed by him for testing the accuracy of reflecting surfaces by immersing them in water? If the surface to be tested is levelled as nearly as possible and just covered by the water, the interference bands produced by the liquid film that stands above the reflecting surface indicate at once whether it is truly plane or possesses any local irregularities. In some cases the simplicity belongs to the problem rather than to the solution, as in the case of the familiar phenomenon of the scintillation of stars (vol. 4, p. 60).

When a subject which at first sight seems capable of easy experimental investigation presents formidable difficulties in the execution, Rayleigh showed his mastery in the design of experimental detail. Take, as an instance, the formula deduced by Fresnel for the intensity of light reflected by a polished surface. When the light is incident normally, the refractive index being μ , the amplitude of the reflected light in terms of that of the incident light should be $(\mu-1)/(\mu+1)$. There is no reason to suspect that the law is not accurate, but it was not in Rayleigh's nature to accept without verification a result, even when obtained by mathematical reasoning that appears to be irreproachable. I know of no problem in the whole range of Physics in which the planning-out of a satisfactory experimental arrangement was beset with so many difficulties overcome in so perfect a manner. The result disclosed a remarkable effect due to exposure to air, which may diminish the reflecting power of glass by as much as from 10 to 30 per cent. without any appreciable tarnishing being visible. A still more surprising fact was observed with a layer of silver deposited on glass, which gave a reflecting power more than 10 per cent. greater on the side exposed to air than on that in contact with the glass.

The limits to which an experimental investigation can be pushed was another of Rayleigh's favourite subjects. He showed how 4 c.mm. of a gas can be made to suffice in order to determine its refractive index; he

investigated the minimum current that could be detected by the telephone, the minimum condensation of air that affected the ear, and the sensibility of the eye to variations in wave-length.

Rayleigh's knowledge of the literature of science was extensive and thorough. In his own papers he invariably acknowledges, sometimes with almost excessive generosity, the work of others. His familiarity with the writings of John Herschel, Young, Lloyd, and authors that are little read, is illustrated in an excellent paper on the "History of the Doctrine of Radiant Energy" (vol. 3, p. 238). It was left to him to point out that Maxwell was the first to establish the equation which embodies the law of anomalous dispersion and is generally ascribed to Sellmeyer. Frequent references to historical matter appear in other papers.

Rayleigh's readiness to supply the explanation of puzzling observations may be illustrated by a short paper on "Coloured Photometry." The established fact here is that the eye has some power of comparing the intensities of luminous impressions due to light of different colours, and some physiological process is required to supply the common standard which makes the comparison possible. The power in question has an important application in the so-called "Flicker Photometer," and Rayleigh's suggestion is that the nerve irritation which causes the contraction of the iris by bright light furnishes the required measure. Two lights would then be physiologically of the same intensity when the tendency to contraction of the iris is the same for both.

Even at times when Rayleigh was most busily engaged in the researches which formed the main object of his activity, he did not lose interest in questions of fundamental importance raised by others. We find him discussing the problem: "Does chemical transformation influence weight?" and showing that an affirmative answer would point to some hitherto unsuspected thermal effects connected with gravitation.

Prof. Michelson's well-known optical investigations suggested the question whether the velocity of light in an electrolytic liquid is influenced by an electric current in the direction of propagation. Experiment gave a negative answer, and similar negative results were obtained on trying to find a possible effect of motion through the æther in causing double refraction, or an influence of the earth's motion on rotatory polarisation.

A man of Rayleigh's power and judgment could not help being frequently called upon to join deliberative bodies in which these qualities are required, and we all know that such external duties are formidable enemies to the conduct of research. Though reluctant to be drawn away from his scientific work, he never withheld his time and energy whenever he believed that he could give substantial assistance. The interests of Science were always placed above his personal inclination; and, especially in earlier years, he did not hesitate to decline a position, however honourable and influential, if he thought he could render better service to Science in his laboratory. In refusing the Presidency of the Royal Society in 1905, he

gave as his reason that he would be as good a President ten years later, while he did not then expect to be equally productive as a man of science.

He was one of the leaders of the movement which led to the establishment of the National Physical Laboratory, and acted as Chairman of the Committee of H.M. Treasury to report upon the desirability of its foundation. On the successful completion of the organisation he accepted the appointment of Vice-Chairman of the Board (the President of the Royal Society being "ex-officio" Chairman), and presided over the Executive Committee until shortly before his death. The services he rendered to the laboratory were invaluable, and greatly assisted the Director, Sir Richard Glazebrook, in raising it to its present unique position among the prominent scientific institutions of the world.

The contributions which Rayleigh rendered to Aviation as President of the Aeronautical Committee have already been mentioned. He was appointed to the Professorship of Natural Philosophy at the Royal Institution in succession to Tyndall in 1887, and held it during nine years. Though not by nature a ready speaker, his lectures were effective, and, like his writings, illustrate the truth of Buffon's saying: "Ce qui se comprend bien s'énonce clairement." He fortunately included Abstracts of his Discourses at the Royal Institution in the 'Scientific Papers': they constitute a rich mine of beautiful experimental illustrations, covering important principles in almost all branches of Physics.

In 1896 he accepted the position of Scientific Adviser to Trinity House. This is an Association of English mariners which received its first charter from Henry VIII as a "Guild or Fraternity of the most glorious and undividable Trinity and of St. Clement." The guild received from Queen Elizabeth authority to erect beacons and other marks for the guidance of navigators along the coasts of England. It was subsequently divided between "Elder Brethren" and "Younger Brethren," the right of sole management being conferred on the former class. After various changes, the practical duties of the Brethren were confined to the erection and maintenance of lighthouses, buoys and beacons, and the supervising of pilots, while the Elder Brethren act as nautical assessors in the High Court of the Admiralty. In the 'Scientific Papers,' we find at least two problems on Sound suggested by Rayleigh's connexion with Trinity House; they both refer to fog signals. The first (vol. 5, p. 133) deals with the necessity of spreading the range within which the sound is heard as much as possible laterally while restricting it in the vertical plane. This requires the opening of the trumpet to be small in the horizontal and large in the vertical direction. That the theoretical conclusion is correct was verified with a trumpet emitting a high note, but it was pointed out that to carry out the idea on a large scale would mean a very large structure, the vertical dimension required for waves 48 inches long being of the order of 6 metres. A memorandum published in 1917 draws attention to the advantage of substituting wireless signals for fog horns on the ground

that these would supply information as to the direction of the origin of the signal.

In 1901 Rayleigh became Chief Gas Examiner, a position which carried with it the duty of arbitrating on disputed points.

A few words should be said with regard to Rayleigh's attitude towards some of the revolutionary notions which were beginning to shake the old foundations of Science. Like Stokes and Kelvin, he was brought up in the faith of Newton's doctrines, but the minds of the three men responded very differently to the first awakening of doubt. Kelvin felt strongly and expressed himself forcibly. To him the mechanism of the æther and of the ultimate constituents of matter could not be otherwise than the mechanism of the gross matter he could handle in the laboratory. That was the Alpha and Omega of his convictions. Stokes was more cautious; where Kelvin was angry, he only smiled mysteriously. In purely scientific matters, and in private conversation or unreported speeches, one suspected that Stokes had something of the iconoclast in his composition, and he enjoyed being told of any experiment that could not be reconciled to current theories. Rayleigh was purely judicial, repressing his personal predilections. To put it into colloquial language, where Kelvin would say: "This is all nonsense," and Stokes answer: "Perhaps; but all the same it is a hard nut for you to crack," Rayleigh would ask: "What is the evidence?" He did not hide his dislike of some of the modern theories, but he went far enough to admit that, if it be impossible to demonstrate experimentally the relative motion of a body and the æther, our belief in the existence of the æther must be reconsidered.

His attitude towards psychical research was mainly determined by his desire that there should be no limitation set to inquiry whatever the subject might be; and that personal prejudice should be excluded from every investigation. The last occasion on which Lord Rayleigh addressed an audience was in April, 1919, when he delivered his Presidential Address to the Society of Psychical Research, his final judgment being summed up in one of the concluding passages of that Address: "I fear that my attitude, or want of attitude, will be disappointing to some members of the Society who have outstripped me on the road to conviction, but this I cannot help. Scientific men should not rush to conclusions, but keep their minds open for such time as may be necessary. And what was at first a policy may become a habit. After forty-five years of hesitation it may require some personal experience of a compelling kind to break the crust. Some of those who know me best think that I ought to be more convinced than I am. Perhaps they are right."

In 1871 the late Lord Rayleigh married Evelyn, daughter of James Maitland Balfour, and sister of the Right Hon. Arthur Balfour. Mrs. Henry Sidgwick, who collaborated in his work on electrical standards, was his sister-in-law. In 1902 he was among the first recipients of the Order of Merit. In 1905 he was made a Member of the Privy Council. From

1892 to 1901 he acted as Lord Lieutenant of the County of Essex. In 1908 he succeeded the late Duke of Devonshire as Chancellor of the University of Cambridge. In 1904 he received—jointly with Sir William Ramsay—the Nobel Prize, the proceeds of which he presented to the University of Cambridge for an extension of the Cavendish Laboratory. It is needless to enumerate the scientific societies which elected him Honorary or Corresponding Member, or the Universities which conferred Honorary Degrees on him. The list would have to include all important scientific bodies in the world.

Among Rayleigh's achievements, the discovery of Argon is that which appeals most strongly to popular appreciation, yet it may be said with truth that his influence on scientific progress is independent of that discovery, which might be eliminated altogether without affecting our estimate of him as one of the greatest leaders of science in modern times. He had not the ambition to initiate revolutionary theories or to advance science by great and sudden strides. His procedure was to bring experiments into harmony with existing theories, to perfect those theories, if necessary, with the help of improved experimental methods, either by extending the range of accuracy or by new lines of investigation. His discovery of Argon was the natural outcome of this method of attack. It might be regarded as being a conspicuous ornament rather than the foundation of a reputation, to which no single incident could do more than add its contributive share.

In supreme devotion to science as well as in the exceptional combination of experimental and mathematical skill there is much resemblance between Rayleigh and Henry Cavendish, and the similarity extends to the advantages conferred by a social position that, in the early stages of a man's career, undoubtedly offers favourable opportunities. The contrast between the two men, therefore, becomes all the more striking when we compare their effective influence on the progress of science. Making due allowance for the inborn shyness of Henry Cavendish, there was an element of selfishness in his attitude which, so far as can be judged from the record of his life, can only be explained by an almost complete absence of human sympathy. Rayleigh's devotion to science was the reverse of selfish. By advice and encouragement he assisted every honest student of science. He liked to hear of the work of others and receive as well as impart information. The scientific discussions at the week-ends spent at Terling Place will always remain in the memory of those who had the advantage of taking part in them, and in the selection of his guests no other consideration ever entered except the common object of their interest in science. No one was ever a more lenient judge than Rayleigh. Even when he criticised he tried to find some mitigating circumstance that would excuse the errors committed, or some solitary idea of value in a work which he was forced to condemn as a whole. He disliked premature publication of discoveries, not only because they did not satisfy his own high standard, but because "scoffers"—as he expressed it—"would be encouraged."

The six volumes in which the record of his researches is collected remain a source of inspiration. The memorial tablet in Westminster Abbey will testify to the esteem of his countrymen, but those who have enjoyed his friendship and acquaintance will place above these tangible monuments the great example he set as a man: his magnanimity, his single-mindedness, the high ideals that have borne fruit in his generation, and may be expected to transmit their fertilising power on those who in years to come will have to carry the banner of science—a science that places humanity above material gain.

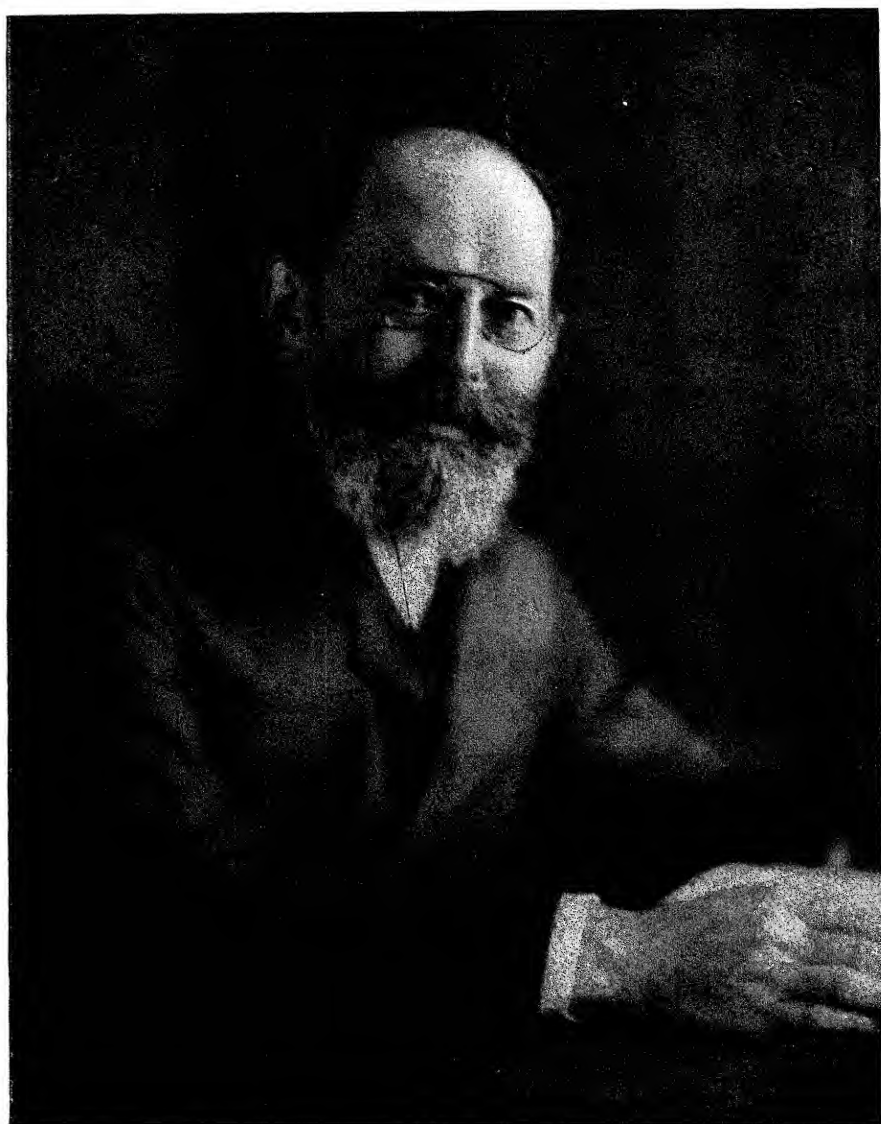
ARTHUR SCHUSTER.

EMIL FISCHER, 1852—1919.

WITHIN reasonable limits of space, and with due restraint of diction, it is difficult to convey an impression of the momentous influence exerted by Emil Fischer upon the chemistry and chemists of his generation. It is impossible to estimate the effect his work will produce in the thought and practice of his successors because, during the forty-five years of his activity, he not only enriched with a wealth of fresh examples and classified with unchallenged insight three fundamental groups of organic compounds, but in doing so he developed the technique and principles upon which is founded the modern science of biochemistry.

The preliminary step, taken in 1875, would then have appeared remote indeed from the goal. In that year he isolated phenylhydrazine from the product of reducing benzenediazonium chloride with potassium sulphite, and the variety of changes undergone by that substance immediately offered ample scope for his industry and ingenuity. During the intervals of studying these transformations he traced, in collaboration with his cousin, Otto Fischer, the hydrocarbon origin of rosaniline colouring matters, showing this to be triphenylmethane (1878), of which, or its homologues, the various eucanilines are triamino-derivatives.

It was in 1884 that Fischer discovered phenylglucosazone, produced by the concurrent dehydrogenation and condensation which take place when glucose or fructose is treated with phenylhydrazine. The significance of this observation lies in the fact that, although when purified the sugars may be crystallised without difficulty, they tend to remain obstinately amorphous when mixed; it follows that an agent which will serve to identify such materials in a sparingly soluble form is a weapon of the highest value in attacking problems involving them. Having demonstrated the utility of phenylhydrazine as a means of identification, he applied it to acrose, the syrup which he



EMIL FISCHER.

and Tafel obtained in 1887 from acrolein dibromide and baryta. This yielded two osazones— $C_{18}H_{22}O_4N_4$, corresponding to the synthetical sugars, α - and β -acrose; the former of these he identified with *dl*-fructose, whilst β -acrose has been since recognised as *dl*-sorbitose. The discovery led to the first synthesis of naturally occurring sugars, and gave the definitive impulse to Fischer's work in the direction of biochemistry.

The procedure by which α -acrose became the source of synthetical *d*-glucose, *d*-mannose, and *d*-fructose depends on the observation (1890) that gluconic and mannonic acids are interconvertible when heated with quinoline at 140° . Applied in conjunction with Pasteur's method of resolving a racemic acid, and associated with the reduction of a polyhydroxylactone to the corresponding aldose, this principle not only enabled Fischer to synthesise the above-mentioned natural sugars, but, conjoined with the cyanohydrin synthesis, led to their optical antipodes and to the isolation of idose, gulose, and talose in their *d*-, *l*-, and *dl*-forms. Bringing the known hexoses into close review on the basis of Van 't Hoff's theory, and assembling the sixteen possible projection formulæ in correlation with those of the eight possible aldopentoses, he proceeded (1891) to allocate the appropriate configuration to each individual, and subsequently (1896) confirmed the choice he had then made by degrading rhamnose to the corresponding methyltetrose and oxidising this to *d*-tartaric acid. Hence, amongst the sixteen optically active aldohexoses theoretically realisable, twelve have been either synthesised or configured, or both, by Fischer and his collaborators; his work on ribose caused *d*-allose and *d*-altrose to be added, and thus the *l*-forms of the two last-named sugars are now the only unknown members of the series. When it is recalled that in 1886 glucose and galactose were alone in the class of pentahydroxyaldehydes, and that the configuration of the four asymmetric carbon atoms was unrevealed, an appreciation of this opening achievement may be gained.

The naturally occurring glucosides, *e.g.*, amygdalin, indican, salicin, and myronic acid are amongst the earliest known compounds of organic origin, but little had been learned of their structure beyond the fact that they yield glucose on hydrolysis. In this sense several polysaccharides are glucosides for sucrose (cane-sugar), lactose, and raffinose all give hexose mixtures to which glucose is common, whilst maltose yields glucose alone. Synthesis of the first methylglucoside by Fischer (1893) was destined to elucidate the structure of glucosides in general, and ultimately to give new information respecting glucose itself. Followed by the discovery of an isomeride by Van Ekenstein (1894), it supported the butylene oxide formula for glucose which had been suggested by Tollens (1883). The α - and β -methylglucoside having been referred to corresponding forms of the parent hexose by E. F. Armstrong (1903), Simon's view (1901) of α - and β -glucose as lower homologues of α - and β -methylglucoside, respectively, was established. Interest in the subject was revived by Fischer's discovery of γ -methylglucoside (1914), with properties diverging from those of its isomerides sufficiently to justify the belief that it is an ethylene oxide, and although the corresponding γ -glucose

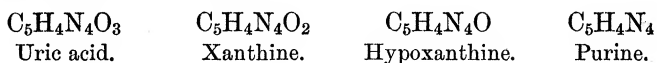
awaits isolation, the recognition of possible cyclic relations differing from that prevailing in α - and β -glucose has brought inquiry to the threshold of a new carbohydrate classification.

By means of glucose derivatives in which one hydroxyl group has been replaced by a halogen and the remaining ones acetylated, Fischer and his colleagues made concrete progress towards the synthesis of polysaccharides and natural glucosides. Whilst sucrose, the original aim of these experiments, was never produced by artificial means, a galactosidoglucose resembling melibiose, a glucosidogalactose differing from lactose, and a glucosidoglucose called *iso*-trehalose, were characterised. Similarly, in the direction of amygdalin synthesis, Fischer and Bergmann (1917) produced mandelonitrile- β -glucoside (sambunigrin and prulaurasin) in both active forms, followed by acetonecyanohydrin-glucoside (linamarin) and glycollonitrile-glucoside (1919), the simplest of the cyanogenetic glucosides.

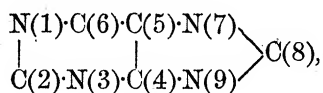
Although not strictly a glucoside, gallotannic acid, or gall-nut tannin, was shown by Strecker (1852) to yield glucose on hydrolysis; but the conflicting results of other chemists had led to the view, prevailing for half-a-century, that it is principally digallic acid. The latter, however, was synthesised by Fischer (1908) and found to be crystalline, although astringent, and having purified the main constituent of Chinese tannin, he obtained definite evidence (1912) that glucose does arise from this by hydrolysis. Improvement in the methods of acylation, necessarily a most difficult operation when involving such sensitive molecules as those of glucose and of digallic acid, enabled him in association with Bergmann (1918) to elaborate penta-(*m*-digalloyl)- α -glucose; the correspondence between potassium salts of this derivative, of Chinese tannin and of penta-(*m*-digalloyl)- β -glucose is so close as to establish the claim that the principal constituent of this form of tannin has been synthesised. The class-name "depside" had been introduced by Fischer and Freudenberg (1910) in application to those anhydrides of phenolcarboxylic acids in which condensation has occurred between the carboxyl of one molecule and the hydroxy-group of another; it was intended to emphasise the resemblance they bear towards tannins in behaviour and polypeptides or polysaccharides in structure, and thus Chinese tannin is elaborated from a glucose molecule in which five hydroxyl groups have been acylated by the depsidic nucleus, *m*-digalloyl.

During the years 1881-1899, Fischer conducted a sequence of investigations into uric acid and allied substances, adding a large number of synthetic products to the series and arranging the members of this important group in a rational system of classification. Uric acid was discovered in 1776, but it was not until 1834 that its composition was established; the constitutional formula advanced by Medicus in 1875 was confirmed by synthesis in 1888, but the relationship with xanthine and hypoxanthine had remained empirical. It appeared to Fischer that caffeine lent itself most readily to experimental treatment and it was consequently with this compound, in conjunction with xanthine, theobromine and guanine that a beginning was

made. Without following in detail the course of his numerous inquiries, it may be stated that his introduction of the word "purine" (1884) for the hypothetical parent of the series,



was most happily justified by the synthesis of purine itself (1898). Basing results upon his notation (1897) of the diureides as proceeding from the bicyclic nucleus



he succeeded at various times in establishing the constitution of xanthine (2:6-dioxypurine), hypoxanthine (6-oxypurine), adenine (6-amino-purine), guanine (2-amino-6-oxypurine), caffeine (1:3:7-trimethyl-2:6-dioxypurine), theophylline (1:3-dimethyl-2:6-dioxypurine) and theobromine (3:7-dimethyl-2:6-dioxypurine), uric acid being 2:6:8-trioxypurine.

Here again may be noticed the absorbing attraction presented to Fischer by the physiological aspect of the materials he selected for study. Purine derivatives include valuable drugs and comprise numerous compounds involved in animal and vegetable metabolism. The principles and processes which he developed in this field have gone far towards consolidating technical procedure in the Merck, Bayer, Höchst and Böhringer factories, whilst in collaboration with Von Mering (1903) he made a substantial contribution to manufacturing practice by an improvement in the production of diethylbarbituric acid, which led to that substance becoming one of the most valuable hypnotics in pharmacy under the name veronal.

The most important consequences of his gravitation towards biochemistry, however, arose from the fruitful inquiries which he made into the structure of proteins, and from the steps which he took in the direction of synthesising albuminoid molecules by associating into polypeptides their units of construction, the amino-acids. It had been recognised that many of these were obtainable from proteins by hydrolytic or enzymic disruption, and several had been synthesised in their racemic form: but Fischer, first suppressing their basic features by acylation (1899) and subsequently emphasising them by esterification (1900), devised practical methods for isolating the optically active modifications and for separating these from one another in complex mixtures. The direct result of these devices was to facilitate the recognition and estimation of component amino-acids in a great variety of natural proteins from widely different sources, of which silk-fibroin, amongst others, received his particular attention. By these means a number of representative amino-acids were assembled, and provided material for attempts to reconstruct the proteins from which they spring; the recognition of hippuric acid as benzoyl-glycine gave a clue to the form of union, and from this foundation a large

series of synthetic products, called "polypeptides" from their resemblance to peptones and, structurally, to polysaccharides, has been accumulated. One of these, an octadecapeptide (1907) composed of 15 glycine molecules and three of *l*-leucine, has the molecular weight, 1213: but refinement in the methods of preparation enabled Fischer to elaborate polypeptides from more diverse units, such as glycyl-*d*-alanyl-glycyl-*l*-tyrosine (1908), isomeric with the tetrapeptide obtained by silk-hydrolysis, and thus to present a link between the products of complex synthetical operations and the peptones arising from incomplete disruption of proteins.

Whilst the simpler polypeptides are crystalline, the tendency to amorphism increases with molecular weight; aqueous solutions of the more complex ones are opalescent, and yield precipitates with ammonium sulphate, phosphotungstic acid, and tannin, thus resembling the proteins themselves. Nevertheless, Fischer recognised the distant position occupied by the synthesis of natural proteins along the path he had begun to follow, because association of the monoamino-carboxylic acids glycine, alanine, valine, leucine, phenylalanine, tyrosine, serine, and cystine in the natural products is complicated by the presence of aspartic and glutamic acids with arginine, lysine, histidine, proline, and tryptophan.

Throughout these inquiries, Fischer made frequent and skilful use of enzymes, developing a technique which will offer invaluable guidance to subsequent investigators of vital changes. In 1894, having assembled a great variety of artificial carbohydrates, he studied their behaviour towards different families of yeast, drawing the fundamental conclusion that the fermentative enzyme is an asymmetric agent attacking only those molecules of which the configuration does not differ too widely from that of *d*-glucose. Applying this principle to the natural and artificial *d*-glucosides, he ranged these in two groups, the α -*d*-glucosides being hydrolysed by maltase and indifferent towards emulsin, the β -*d*-glucosides exhibiting converse behaviour. The *l*-glucosides, *d*-galactosides, arabinosides, xylosides, rhamnosides, and glucoheptosides were not affected by either enzyme, and the glucosidic relation of sucrose, maltose, and lactose was determined by similar means. It was the knowledge thus gained which led Fischer to represent enzyme-action by the analogy of a lock-and-key, and to conclude that disaccharides are fermented only as a consequence of preliminary hydrolysis.

Turning his attention to secretions of animal origin (1896), he studied the behaviour of carbohydrates and glucosides towards blood serum and a great variety of tissue-extracts and juices, but it was when those agents were applied by him, in association with Abderhalden (1903), to the proteins and polypeptides that the most fruitful results arose, from which it followed very clearly that the synthetic polypeptides are susceptible to zymolysis only when constructed of those amino-acids which occur in the natural proteins.

Emil Fischer was born on October 9, 1852, at Euskirchen. The only son of Laurenz Fischer and Julie Poensgen, he seemed destined for a career in

commerce, his father being a flourishing merchant and associated with his uncles in other ventures. Accordingly, on leaving school at Bonn in 1869, he was apprenticed to his brother-in-law, Ernst Friedrichs, a timber-merchant, but preference for experimental science led to his becoming a pupil of Kekulé in 1871. In the following year he proceeded from Bonn to Strassbourg, where he graduated in 1874 under Von Baeyer, passing with him to Munich in 1875. There he succeeded Volhard in 1879, and again in 1882 at Erlangen, whence Volhard had been called to Halle. On the transference of J. Wislicenus to Leipzig in 1885, he was appointed to the Chair of Chemistry at Würzburg, and, when Von Hofmann died in 1892, became Professor and Director of the Chemical Institute in the University of Berlin, occupying this post until his death, which took place in the night of July 14, 1919.

Even the remarkable diligence and insight which were his could not have enabled him to compass a field of achievement so vast, had he not possessed in full measure the quality of arousing enthusiasm in his collaborators, which followed inevitably from association with his kindling personality. Students from many nations were attracted to his laboratory in large numbers, and, although the prevailing atmosphere gave slender encouragement to aught else work, his quickening zeal did not fail to transform willing labourers into warm admirers. This was hastened by knowledge that his untiring singleness of purpose and somewhat austere manner were tempered by love of music, of art, and of joyous, human relaxation. His practice of concentration, stimulated at first by attachment to his chosen pursuit, was encouraged by the need to economise physical energy in consequence of suffering from the poisonous effect of mercury diethyl, and afterwards, more seriously, of phenylhydrazine; at a later period, absorption in work had to serve as a palliative to grief in the loss of his wife, Agnes Gerlach, who died in 1895, after a happy union of seven years' duration.

Fischer received the Davy Medal in 1890 and was elected a Foreign Member in 1899. He was awarded the Nobel Prize for Chemistry in 1902, and in 1907 delivered the Faraday Lecture before the Chemical Society, of which he had become an Honorary and Foreign Member in 1892. The address which he then gave, entitled "Synthetical Chemistry in its Relation to Biology," was a memorable synopsis of biochemical principles. Therein he traced the early connection between organic chemistry and those products of animal and vegetable life-processes which had provided Lavoisier, Gay-Lussac, Berzelius, and Liebig with material in elaborating the methods of chemical analysis; he showed how, later in the nineteenth century, organic chemistry had become separated from biology, the development of the subject by Liebig's famous pupils, Von Hofmann, Kekulé, and Wurtz, having proceeded on other lines, and he indicated the manner in which the association of the two subjects had become restored. Outlining a possible mechanism by which sugar may be produced from carbon dioxide through the agency of chlorophyll, he dwelt on the ignorance which still must be confessed

regarding synthesis of the fats from carbohydrates in plants or animals, and as to the manner in which they undergo combustion to carbon dioxide and water in the animal system. Finally, dealing with proteins, the most complicated of all materials concerned in cell-activity, he traced their relationship to peptones and amino-acids, explaining how the latter arise by various methods of hydrolysis and may be re-assembled into products which, in their simpler forms, resemble peptones, and even approach the proteins as their complexity increases. At appropriate points he emphasised the part played by enzymes in the catalytic changes which digestion and assimilation involve, the whole treatise constituting a powerful plea for renewing the alliance between chemistry and biology, an alliance which his own work has done more than that of any other chemist to cement.

The subjects to which Fischer mainly devoted his attention were not related directly to problems of manufacture, but he soon made contact with the chemical industry, and in 1883 was chosen to succeed Caro as Director of Research in the Badische factory. Although this most attractive offer was refused, he occupied towards the industrial leaders a very exceptional position, which was quite as much due to his own remarkable qualities as to the German system of linking science with technology. It was this which helped him to become instrumental in establishing the Kaiser-Wilhelm-Institut für Chemie, a foundation devoted to the pursuit of chemical research independently of teaching duties; the ceremony of opening the laboratories at Dahlem, Berlin, took place in October, 1912.

The variety of chemical issues engendered by the War absorbed his attention with increasing urgency until the last few months of his life. Foreseeing the effect of the blockade on the production of nitric acid from Chile saltpetre, he took part in the measures adopted for increasing the output of ammonia from coke-ovens and in the development of the synthetic nitric acid industry; but for the dimensions attained by the latter enterprise, Germany could not have continued the struggle after the middle of 1915. Problems connected with the need for augmenting the supplies of benzene and toluene, for utilising the sulphur-content of gypsum, and for seeking alternative sources of glycerine, also engrossed him in the earlier years; but it was the food-shortage, with all its physiological and psychological consequences, which ultimately became the most threatening element in the domestic situation. The commission appointed at his instigation explored the possibilities of straw, wood, leaves and rushes in supplying fodder for horses and cattle, and he devoted special attention to finding a coffee-substitute.

Burdened by these exacting labours and their distressing futility, his power of resistance to the malady from which he suffered became undermined. The eldest only of his three sons now survived, but the laboratory continued to be a source of mental distraction; in it he remained productively employed to the end. In view of the great age attained by his father, who lived to be 94, a period of activity longer than the 67 years

allotted to Emil Fischer might have been anticipated for him ; but organic chemistry has never stimulated a more devoted follower, or been endowed more richly and permanently with the fruits of devotion to its pursuit.

Nevertheless, his roll of practical achievement is not the only basis for the admiration of his contemporaries. From boyhood a pronounced individualist, he rapidly acquired a grasp of essentials which, as he rose to eminence, gained him a position of unlimited influence amongst the scientific men of Germany. Trusting personalities more than organisations, and wisdom more than learning, his own magnetic personality and convincing wisdom were freely applied in the furtherance of scientific method, both industrial and academic. Whilst always in sympathy with the requirements of chemical industry—and herein lay one important factor of his authority—he placed the sound training of chemists, and the practice of investigation for its own sake, foremost in his creed. Such great qualities need to be recorded, because they tend to become obscured by the passage of years and the removal of those whom they inspired to action ; but the glowing record of his additions to exact knowledge will remain undimmed by time.

M. O. F.



Raskin



EMIL FISCHER.